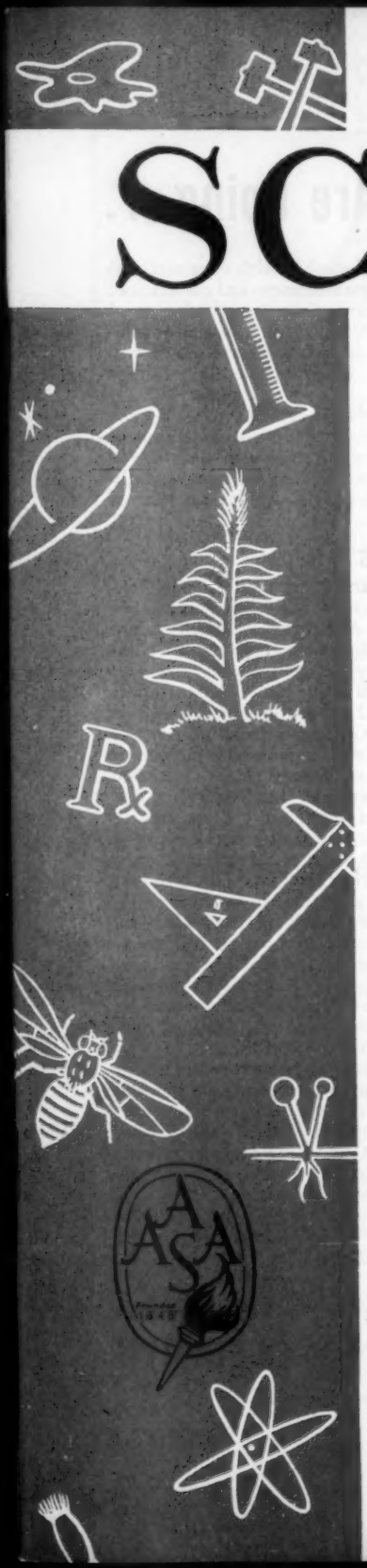

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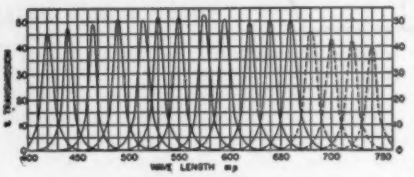

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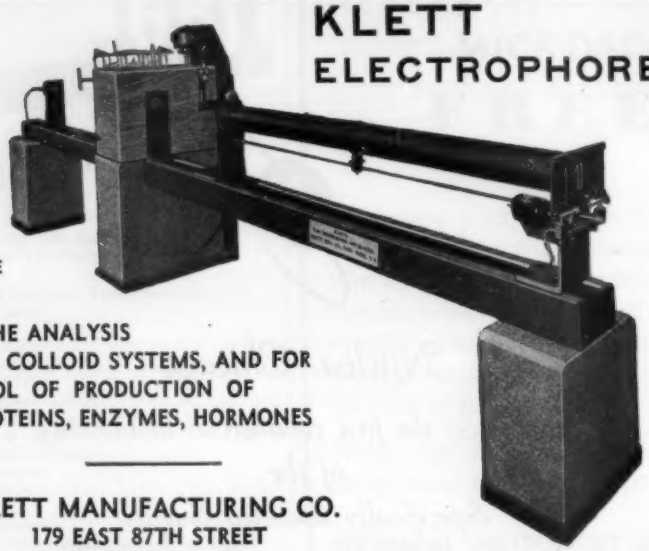

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Is the Clinical Practice of Psychology a Science?

THE question "Is psychology a science?" is not a new one. The members of the psychological profession continue to disagree on the answer. Sometimes, I am afraid, it is rather intense disagreement. Those psychologists who consider themselves to be primarily clinicians will insist that the practice of psychology is, to an important extent, intuitive. They see psychology as many physicians view medicine, primarily as an art in which much is bound to elude scientific method. However, other psychologists insist that their own orientation requires verification of all data with which they deal. Some are outspoken in their criticism of methods that employ the so-called "subliminal" clues. Among these are the statistical, experimental, physiological, and perhaps the industrial psychologists. Therefore, the answer given to the question—Is psychology a science?—depends on who is consulted.

I believe that such evidence as would be available to the clinical psychologist must, for a long time, be gathered in what may not be an orthodox, scientific manner. The proof that would be necessary to validate some of the tools of the clinical psychologist requires extensive longitudinal studies, such as the Air Force is now conducting in the psychiatric screening of aviation cadets. It is easy to insist that clinical psychology relinquish its "unscientific assumptions" and return to the fold of "true science." However, if clinical psychology is not to go out of existence altogether, either it must continue as it is or the means for scientific inquiry and investigation must be provided. This entails a considerable amount of work, trained personnel, facilities, and, most important of all, time. Very, very few financial grants for such all-inclusive studies are available.

It would be fortunate if this editorial would reach the receptive ears of some individual who is in a position to allocate sizable funds for such human progress, although I am aware of the fact that other areas of investigation have an equally valid claim for sponsorship. If funds were available, I would recommend the administration to the half-dozen psychological tests most frequently used for the prediction of human behavior, and a few of the promising new techniques, to an experimental cross section of a representative American community. Regular follow-ups of the activities, adjustment patterns, and mental health of the psychologically tested subjects in later life would be required to obtain criterion data and check hypotheses. Rather than validity studies of individual tests—as is the common practice now—a psychological battery of tests should be treated comprehensively as part of an integrated psychological examination. What is needed is a psychological equivalent to sociology's Middletown.

Most clinical psychologists join their colleagues in the belief that what exists, exists in some quantity and can be measured. They agree, further, that it should be measured and assumptions should be verified. It is proper to ask, however, whether valid measurement is possible under the very limited experimental conditions presently available to investigators in this area. Does human personality with its infinite complexity lend itself to atomistic analysis or statistical appraisal? Are the little experimental forrays that presently constitute the bulk of clinical psychological research really meaningful?

Psychology *can* be a science. Much of what is called psychology is scrupulous in its adherence to the scientific method. Other areas—especially personality appraisal and prediction of human behavior—require a comprehensive program of investigation and experimentation before they can be accepted into the ranks of the disciplines that undeviatingly follow the scientific tradition. In the meantime, as far as these areas of psychology are concerned, good art may be preferable to poor science.

THEODORE C. KAHN
USAF School of Aviation Medicine,
Randolph Air Force Base, Texas

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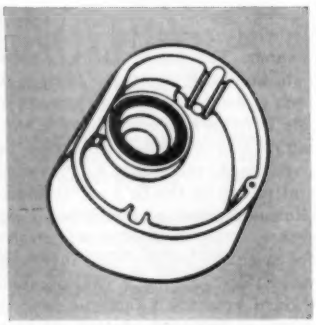

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Science and Modern Cosmology*

Herbert Dingle
University College, London

IT has become the custom in recent years for the president, during his term of office, to address the society on the department of astronomy in which his own work lies. It is a good custom, but not one which at first sight it seems possible for me to maintain. My subject is the history and philosophy of science, and that can scarcely be called a department of astronomy; it seems either to lie wholly outside or else to include astronomy as a small part of its concern. In neither case does its claim to your interest on this occasion seem a strong one. Nevertheless, I believe that it is in fact highly relevant to the present position in astronomy. Had that not been so, I should not have been justified in accepting the high office to which you called me 2 years ago. I propose therefore to explain as best I can why I think this subject is vitally pertinent to the working astronomer of this age.

Of course, in one sense the study of the history and philosophy of science needs no apology in any sphere. Science plays such an essential part in modern life, on both its active and its contemplative sides, that no one can call himself educated who has not some idea of its nature and its history. That I think is indisputable, but it is not so clear that a knowledge of these things is necessary to the actual prosecution of astronomical studies. Science has been advanced in the past by men who have paid little attention to the wider implications of their actions, men who would have been unable to formulate any clear statement of what they were doing, and whose knowledge of the origin and growth of science has been negligible. They have gone forward in the right direction as though by instinct, seeing spontaneously what is legitimate and what is not. And such men have not been exceptional among scientists; they have been typical. It is not surprising, therefore, though it is regrettable, that there are many even today who hold that the scientist and the philosopher of science are pursuing independent ends, and the less they interact, the better for the progress of science.

The apparent justification for this view lies, I think, in the peculiar nature of science by which in its earlier stages it has automatically generated within itself its own steering mechanism, so to speak. In the first place, the world has been so full of mysterious phenomena that there has been no temptation to wander outside experience in search of other realms to conquer; and second, it has become almost an intuition with scientists that they must submit their findings to the test of observation at every stage. If a scientist tends to postpone this duty, his neighbor, who holds different views, has always been at hand to perform it for him. Science has thus stood in little danger of violating its own essential principles, and the understanding of what those principles are has been, from the practical point of view, a superfluity. But this state of innocence no longer exists. In cosmology in particular it has almost entirely disappeared. The universe we contemplate today is no longer the observable world that for 2000 years was the sole object of astronomical study; it is a hypothetical entity of which what we can observe is an almost negligible part. The assertions we make about it, if susceptible of test at all, require observation over periods of millions of years or in the remote past and are, therefore, beyond any sort of practical check. In these circumstances, there is nothing that can control speculation, and preserve legitimate theory from idle fancy, but a strict adherence to the essential principles of science, those principles that in the 17th century started the course of ever accelerated progress by which the scientific philosophy is most obviously distinguished from the philosophies that were then its rivals.

In cosmological studies, then, a knowledge of the history and philosophy of science is not a superfluity; it is a necessity. I therefore make no apology for asking you to consider its bearing on the recent revival of interest in this most ancient branch of our subject. I do so the more readily because, although some of the prominent contributors to modern cosmology have unconsciously realized that the nature of science has some special relevance to their researches—witness their frequent appeals to what is "scientific" and what is not, which are not a normal feature of astronomical papers—they betray not only a profound ignorance of the subject but also a clear lack of any serious reflection on what they are saying. It is as though—as, in fact, I believe to be the case—they are not really concerned to know whether they are being scientific or not but wish to dignify their own opinions, and discredit opposing ones, by invoking a name that commands general respect. Let me take two examples.

"It is against the spirit of scientific inquiry," says one writer (*1*), "to regard observable effects as arising from 'causes unknown to science,' and this in principle is what creation-in-the-past implies." There are more than one astonishing aspects of this remark. For instance, what conceivable misfortune could have led the writer so to misinterpret both the words and the performances of almost all the great contributors to science as to think that a confession of ignorance is "unscientific," when in fact one of the chief characteristics of the scientific attitude has always been just that particular type of humility. Galileo recommends his readers "to pronounce that wise, ingenious and modest sentence, 'I do not know'" (*2*). "The cause of

gravity," wrote Newton, "is what I do not pretend to know" (*3*). And again (*4*)

> . . . to derive two or three general Principles of Motion from Phaenomena, and afterwards to tell us how the properties and actions of all corporeal things follow from those manifest Principles, would be a very great step in Philosophy, though the Causes of those Principles were not yet discover'd: And therefore I scruple not to propose the Principles of Motion above-mention'd, they being of very general Extent, and leave their Causes to be found out.

I refrain from running through the centuries to cite confirmatory evidence, because I must mention a still more astonishing thing. After making this unfortunate remark, the writer proceeds to develop his own idea, which turns out to be precisely what he has just opposed to the spirit of scientific inquiry—namely, that the observable universe was created in the past by causes unknown to science. The natural inference is that he is averse from scientific inquiry and is arguing for a more excellent way, but I do not think that this is the true explanation. I believe that he has a vague feeling that he can confer some dignity on his theory if he can stigmatize alternatives as "unscientific," and without reflecting on what science really is, or on the obvious implications of his own remark, he presses its name into his service for that end.

I pass to my second example. "Experimental science," we read (*5*), "is based on the assumption that the repetition of an experiment will reproduce the original results, and indeed the realm of experimental science is defined by this criterion." And again, elsewhere, to emphasize the point: "The unrestricted repeatability of all experiments is the fundamental axiom of physical science" (*6*). The first thing to notice about this is that experimental science is said to be based on an assumption. It is based on no assumption; it is an adventure, in which you accept whatever you find, and although you may be guided in a particular case by an expectation, the experiment may reveal something totally different. Schwabe counted sunspots with the idea of finding an intra-Mercurial planet; instead he found the 11-year solar period, and he would have found exactly the same had he counted the spots with any other object in view, or with none. Does anyone believe that if tomorrow it is discovered that some assumption—any one you like—which has been made in the past is baseless, the achievements of experimental science will suddenly lose all significance? I do not think so. The "fundamental axiom" goes wrong at the very beginning. But not only is it not the basis of science; it is patently false and could not be the basis of anything worthy of acceptance. The most striking example of this is afforded by psychology. It would scarcely be wrong to say that in this science—the emergence of which in recent years is one of the most striking relevations of the great scope of the scientific philosophy—there is no experiment which, when repeated, produces the original result (*7*). It would be difficult to conceive of a more crippling restriction on the liberty of modern scientific inquiry than the adoption of this supposed fundamental axiom. But, indeed, one need not go to psychology for evidence. Even in physics, where the writers might have been expected to be more widely informed, it has been the universal belief for the last hundred years that no experiment is repeatable, the entropy of the universe never being twice the same. It can hardly be objected that the universe is irrelevant, because it is precisely to the universe that the supposed axiom is later to be applied, but even on the small scale there are sufficient examples of hysteresis phenomena to have shown the falsity of such a notion to anyone who had stopped to think for a moment. Two apparently indistinguishable pieces of iron are subjected to the same magnetic field and they respond differently; and the explanation that science gives is that one has been through the process before and repetition gives a different result. In the circumstances it is perhaps not superfluous to say that if it is contended that the pieces of iron are really different, but only appear alike, the whole argument becomes circular. The reason why so many experiments are approximately repeatable is that we take infinite pains to select them from the others, because in the earlier stages of science, in which we are at present, they are easier to interpret than the great majority of possible ones which are scarcely ever repeatable. Our cosmologists have mistaken a common-sense device for an essential principle.

Now I have not given these examples merely to show that the name of science is treated with a casual disrespect unworthy of serious investigators. If that were all they could be ignored. But the position is much worse than that. This last illusion is not merely a casual aside. It is made the basis, and the sole basis, of a sweeping assertion which has been called "the perfect cosmological principle," for which, with its companion, "the cosmological principle," enormous claims are made. For example, the cosmological principle has attained the "pre-eminent position in cosmology" (*8*), and "In any conflict between general relativity and the cosmological principle it seems that it is general relativity that must be abandoned" (*8*, p. 122); while at the behest of the perfect cosmological principle atoms are created and nebulae vanish from the observable universe. The "cosmological principle," I should explain, is the assertion that "the universe presents the same aspect from every point except for local irregularities" (*8*, p. 11), and the "perfect cosmological principle" states that "apart from local irregularities the universe presents the same aspect from any place at any time" (*8*, p. 12). If we ask for evidence for these principles, of the kind that can be accepted as scientific, we see that for the "cosmological principle" there is the fact that so far as the universe has been surveyed—which is probably a very small fraction of its whole extent—there is a rough approximation to uniformity of distribution of matter. This makes it reasonable to assume, for purposes of investigation, that a similar uniformity might characterize the whole, in anticipation of the time when we can

compare the implications of the assumption with the wider knowledge that will later become available. For the "perfect cosmological principle," on the other hand, there is no evidence of any kind at all. Since it causes me considerable discomfort to use names which are clearly misleading, I shall refer to the "cosmological principle" as the *cosmological assumption* and to the "perfect cosmological principle" as the *cosmological presumption*, reserving the right, when the "absolutely perfect cosmological principle" makes its appearance, to introduce the terms *first* and *second* cosmological presumption.

Now we have here a remarkable and a very serious phenomenon. I have no time to discuss the meaning of science, so I will here assert of it only that which I think will command universal assent, namely, that *no statement about the universe, or nature, or experience, or whatever term you prefer for the object of scientific investigation* (*9*) *shall be made*—let alone advanced as a fundamental principle—*for which there is no evidence*. What we are faced with now is the quite different claim that *any statement may be made about it that cannot immediately be refuted*. It seems that if you are attracted by an idea for which there is no evidence, all you have to do is to call it a "principle," and then no evidence is needed. We are told that matter is being continually created, but in such a way that the process is imperceptible (*8*, p. 143)—that is, the statement cannot be disproved. When we ask why we should believe this, the answer is that the "perfect cosmological principle" requires it. And when we ask why we should accept this "principle," the answer is that the fundamental axiom of science requires it. This we have seen to be false, and the only other answer that one can gather is that the "principle" must be true because it seems fitting to the people who assert it. With all respect, I find this inadequate.

I want to trace the steps by which it has become possible for what is in fact the contrary of science to present itself as something essentially scientific, and not to be immediately recognized in its true form, but before attempting this I must call attention to another example of the same disconcerting phenomenon, for both will be found to spring ultimately from the same source. I said that in true science nothing is asserted without evidence. I can go a step further, without challenge from any authority of whom I have ever heard, and say that the object of scientific investigation is the world of experience—or, if you like, experience itself—and that all advances in science consist either in enlarging the range of experience or in expressing the regularities found or to be found in it. The new doctrine which I have so far been discussing arises from the substitution for the phrase "expressing the regularities found in experience" of the incompatible phrase "asserting that experience must conform to the tastes of the investigator"; and the other doctrine, which is its twin, arises from the substitution of the phrase "asserting that experience must conform to a pattern which we can deduce by pure reason." This also has been used to build up an elaborate scheme of cosmology (*10*), and it deserves a little closer examination before we seek to discover how it came to intrude into the scientific domain.

This doctrine is usually expressed in the form that the laws of nature can be derived by reason without recourse to experience (*11*). What those who assert it seem to overlook is that, by definition, laws of *nature* are laws followed by that which we experience, not that which we deduce from our axioms. It is therefore self-contradictory to say that laws of nature can be derived without recourse to experience: it is like saying that eggs can be eaten without recourse to eggs. What seems to be meant is that there are some objectively existing mathematical formulas called "laws of nature" which we can arrive at by a process of reasoning, and then, if we look at the world, we shall find that it behaves according to those laws. But suppose it does not, as even the simplest of us may imagine? Only one of the believers in this doctrine, so far as I know, has attempted to answer this question, and his answer is that in that case we must have made a faulty identification between the *a priori* laws and the corresponding facts of nature (*12*). But this leaves us no means of knowing when we have made the right identification except by an appeal to experience, so we are back at empirical science again; we can derive laws of nature without recourse to experience, but we cannot know that they are laws of nature without recourse to experience.

The fact is that nothing at all can be derived by reason unless you start with certain axioms which in the last resort must be chosen arbitrarily (*13*). Take what seem to be the most obviously necessary of *a priori* truths—the rules of simple arithmetic. Nothing would seem more clearly a truth derived by pure reason than the statement $1-1=0$, and to this, if to anything, we might expect experience inevitably to conform. But does it? In some things, yes, and in others, no. If you take from me my only sixpence, I have none left; the law is inexorable with regard to sixpences. But if you take from me my only idea, you are (let us hope) enriched, but I am not impoverished; I may even acquire another idea in the process; $1-1=$ at least 1 in that case. The statement $1-1=0$ is a truth derivable from certain axioms about numbers, and from other axioms you can derive other conclusions, which you can call "laws of nature" if you wish and which are equally necessary deductions from the premises. But the scientific question is not whether you can derive such laws about the universe, but "Is the universe the sort of thing that obeys those laws or is it not?" You can answer that only by observing it to see.

We have, then, the strange position that in cosmology two impostors have usurped the throne of science, worn her crown, and taken her name. Whereas the source and final court of appeal in science is experience, that of one impostor is personal taste, and that of the other, pure reason. Neither is, of course, new: it was one of the triumphs of the scientific philosophy in the 17th century to have apparently routed them both.

But they differ in this respect, that the former has never before, to my knowledge, presented itself quite so brazenly as a genuine philosophy; it has always worn a disguise showing a more respectable aspect. How has it come about that at this relatively advanced stage in the progress of science these pseudo-sciences have again come to life and threaten to deceive the very elect?

I think the process began with the advent of the theory of relativity. This is ironical, for the essence of that theory lay in the recall of science to experience from which it had unconsciously strayed. Nevertheless, I think we can see how the thing happened. As everyone knows, the solution of certain problems which faced classical physics was found to lie in the fact that some concepts—the absolute simultaneity of separated events is perhaps the best-known example—had been assumed to be directly related to experience, whereas in fact they were not. On examination it proved that by no physical process was it possible to determine unambiguously whether two such events occurred at the same time or not. Accordingly, in order to establish science on its proper basis—that of experience—such concepts as these were eliminated and experience was described in terms of those concepts alone which had a precise empirical meaning.

The particular error which was thus corrected happened to arise from incomplete consideration of the effects of changing one's coordinate system. Now a coordinate system is an invention of the scientist which facilitates the expression of the regularities which he finds in experience. It is not itself something found in experience; it is a purely imaginary construction which some scientists have tried to do without, but which nevertheless has become firmly established as a part of the physicist's language, so to speak. Before describing physical events it is convenient to choose one particular point in space and one particular instant in time, and to assign to them the number 0. The point-event thus defined is called the *origin* of coordinates. Next, three mutually perpendicular lines (or something equivalent) are imagined to extend indefinitely far, each in both directions, from the origin. These are termed the *axes* of coordinates. They are graduated in any arbitrary way, provided that the graduation numbers increase numerically outward from the origin in all directions; and the successive instants of time are enumerated in any arbitrary way, provided that the later of any two instants has an algebraically higher number than the earlier. Finally, the whole system of coordinates is assumed to be moving with some velocity v in some direction. Now every element of this coordinate system—the origin, axes, graduation, velocity—is arbitrary, to be chosen as the investigator thinks convenient. It is a fiction, a language in terms of which to express what is observed to happen, but it is itself not something that exists or happens, not a part of the universe or an element of experience. It follows that you can change it as you wish, without affecting in any way at all what is to be described in terms of it, but of course changing the description, just as you do not change the meaning of a sentence when you translate it from English into French, although you change the sounds uttered or the marks made on paper.

Because of this essential arbitrariness it becomes important to understand how to translate descriptions of experience from the terms of one coordinate system into those of another, for only thus can you distinguish, in a particular expression of the laws of nature, what part belongs to the world described and what merely to the language of description. The essence of the theory of relativity consisted in the discovery that it had been wrongly assumed in the past that one particular set of coordinate systems had a unique importance in that it was not, like the others, merely more convenient than its rivals, but was itself a part of nature. All efforts to discover this unique set had failed, said the theory of relativity, because there was nothing to discover, and to safeguard future investigators from making the same error it employed a particular mathematical device, known as the tensor calculus, which automatically insured that laws of nature expressed in terms of it were exactly translatable from any one coordinate system into any other. From that time onward it became the fashion to seek a tensor expression for all laws of nature. Indeed, in some respects the thing was overdone, for while it is true that tensor expressions insure that no coordinate system has preferential treatment, they are not necessarily alone in this, and a proposed law of nature is therefore not necessarily invalid if it is not in tensor form. That, however, is a minor point. The essential thing is that by means of the tensor calculus we can avoid the mistake of ascribing to nature what actually belongs only to the arbitrary machinery for describing nature.

In carrying out the program thus set up, Einstein arrived at what have been called his "field equations"; they are as follows:

$$G_{\mu}^{\nu} - \tfrac{1}{2} g_{\mu}^{\nu} G = -8\pi T_{\mu}^{\nu}.$$

These are expressions in tensor form of certain regularities found in nature by experiment. The distinction between the expression and what is expressed is perfectly clear. On the right-hand side of the equations you have the relations which have been found by experience to hold between measured quantities. These could have been derived in no other way than by observation; they are direct statements of the results of observation, and as such are independent of any freedom of choice which the scientist may possess—other than that of refraining from making or examining the measurements, of course. On the left-hand side of the equations you have the expression of these relations in terms of coordinates, and by choosing a tensor expression you insure that if it holds good in one it will hold good in all such systems, whatever they may be. The observer's liberty is thus wholly confined to the left-hand side, and it is subject only, but inescapably, to the condition that his tensor must in fact express the observations given on the right-hand side. In general, there are a number of tensors that will do this.

Pending further knowledge, the observer then chooses the simplest and, if he is wise, remembers that his choice may be wrong. This is what Einstein did, and the essential condition of science was thus preserved. The findings of experience stand first of all in their own right, and the mechanism of description is then chosen with complete freedom so long as that right is not violated. This is known as the general theory of relativity.

At this point it becomes possible to apply the theory to the universe. The first step is to observe the universe and see how it behaves; the second, to find tensors whose mathematical properties correspond to that behavior. Unfortunately, the first step is possible only in the most rudimentary form. We can observe a little of the region nearest to us; of what lies beyond we know nothing. It is necessary, therefore, if we are to consider the matter at all, to make some assumptions. Einstein assumed that the universe was homogeneous; that is to say, that the region we can observe is a typical sample of the whole. He recognized, of course, that this was an assumption, but the other possibilities were so numerous that it was impossible to choose between them otherwise than by pure caprice, so the assumption of homogeneity was made to see what it would yield. The field equations could then be used to deduce what happened at the places and times corresponding to high values of the coordinates, at present beyond the reach of observation. The deductions would be valid, subject to two conditions: first, that the universe was, in fact, homogeneous, and second, that the tensor chosen for the left-hand side of the equations was, in fact, the right one, so that extrapolation could be made with safety.

I need not describe how Einstein's original model for the universe came to be modified when the systematic recession of the nebulae was discovered, because this introduced no change of principle. What I am seeking is the process by which this essentially scientific procedure became degraded into unscientific romanticizing. I think the first step on the slippery slope was taken when, in the spate of popular expositions of relativity that followed the eclipse observations of 1919, a coordinate system—a somewhat abstruse mathematical conception—was transformed into an "observer," whom the general reader was presumed to be able to picture much more vividly. Instead of comparing two coordinate systems which were equally at the choice of a single observer, two observers were compared, each of whom was supposed to regard himself as being at rest at the origin of his own unique system. I do not think that those who adopted this device can be blamed for its consequences. It is certainly more picturesque than the naked truth, and, provided that the hypothetical observer is granted no properties that are not possessed by a coordinate system, it is quite legitimate. The expositors could scarcely have foreseen that the coordinate system was soon to be endowed with a mind, with superhuman observing faculties, and with authority to dictate to the poor working scientist what kind of instrument he must restrict himself to and how he must adjust it. But that, in fact, is what happened. In the theory of kinematical relativity (*14*) we do not start with observation; we start with a universe full of supposititious observers who must not only be able to construct instruments but must voluntarily choose particular numerical constants—that is, they must be thinking beings, not merely mechanisms. These observers are essential to the existence of science, for they are necessary to the construction of all measuring instruments; unless they exist and behave in the way laid down for them, science cannot begin. The mere fact that it has begun without their assistance is ignored. The instruments we have in fact used are proscribed because, it is said, they cannot be "defined." The beautifully clear-cut division that relativity made explicit between the world to be observed and described on the one hand, and the coordinate language in which to describe it on the other, is completely obliterated. The coordinate language is transformed into a multitude of intelligences and projected out into the world to be observed. We do not in fact observe them, but that does not matter, for we have already decided how they behave. Our part is to restrict our operations to those which they approve, and the rest of the universe must be such that they all agree about it. If it appears to us to be different from that, so much the worse for us; our observing instruments must be illegitimate, and no account must be taken of their findings.

This seems a long way from the scientific ideal of accepting observations without question and reducing them to order by the free operation of reason, but I think we can see how it has happened. It is true that general relativity made a sharp distinction between the world to be observed and the coordinate systems of our invention, but that distinction was very different from what had previously been supposed. Scientists had long been accustomed to choosing the origin and axes of their coordinate system when and where they liked, but they had always supposed that its motion was not at their choice; it belonged to the world outside. Relativity showed that that was not so, that just as it was impossible to say that one particular place and time afforded the only legitimate observation point, so it was impossible to say that one particular state of motion was so distinguished. A great deal, therefore, of what had previously been thought to belong to nature was shown to belong really to ourselves. Figuratively speaking, Einstein's field equations could be said to have been formed out of their predecessors by a huge transfer of substance from the right- to the left-hand side. This sudden accession of importance to the coordinate system predisposed mathematical physicists to invest it with still greater importance, and its personification as an "observer" pointed the direction of development. It was automatically animated and given rights of its own which the actual observer was not allowed to violate. His instrument thus became a Frankenstein's monster, slipped from his control, and began to dictate to him what he should observe. And this was done in the name of relativity (*15*). It was

completely overlooked that the justification for Einstein's action lay in the fact that no physical effects of motion in itself, of "absolute" motion as it is called, were discoverable and that its transfer from nature to the coordinate system was therefore a simple expression of fact. To satisfy the demands of the hypothetical observers, *relative* motion also was transferred to the coordinate system, although the effects of this were observed every day. All that was necessary was to call the time of occurrence of these effects by the symbol τ instead of t, and the thing was done. The imaginary observers were in complete control, and science, its name having been purloined, was left as an outmoded superstition.

It is now but a step to the advent of the second of our two usurpers. The whole foundation of kinematical relativity lies in the existence and potentialities of the army of cosmic observers. They must therefore be established on an unshakable basis. They are certainly not observed; hence they must be either imagined or reasoned into existence. Imagination had not then become sufficiently respectable as a basis for cosmology—that was to be reserved for the creators of the cosmological presumption—so it was held that the cosmic observers were required by pure reason. A universe that did not conform to the behavior prescribed for them was held to be irrational. This universe is not irrational. From this the whole scheme followed, with far-reaching consequences, not only astronomical but even theological (*10*).

It is very difficult to describe this work objectively without giving an impression of satire, but I have no intention whatever of doing so. I have the extremest admiration for the single-minded devotion and superlative mathematical skill which are evident throughout the development of this grandiose scheme, but I find it impossible to describe it in the light of the accepted principles of science without making it appear fantastic. And the reason is simply that it is fantastic. Repeated attempts to call attention to what appears as its fallacies have been futile. They have never been answered, but have simply been dismissed as "trivial" or even "frivolous" (*16*). No criticism has been admitted as valid that is not a criticism of the internal consistency of the scheme. The status of its foundations, and its relevance to the traditional object of scientific inquiry, the world of experience, are held to be idle questions. I can only do my best to present it in the clearest possible light against the scientific background, and leave it to be judged in that setting.

I return now to the other violation of science in modern cosmology—that which originated in the cosmological presumption. This also was made possible by the revolution in thought caused by the general relativity theory, and its point of departure is again the wide scope shown to be possessed by coordinate systems. Let me point out once more the fundamental distinction which general relativity acknowledged and emphasized between coordinate systems, which are entirely subjective, entirely under our control, and entirely independent of the universe which they are employed to describe, and the universe itself, which is entirely objective, not in the least degree under our control, and remains precisely the same whatever coordinate system we employ for describing it. Kinematical relativity made the error of personifying coordinate systems and so endowing them with properties that could properly belong only to the objective universe. The cosmological presumption made the opposite error. It transferred to the universe the wider characteristics which relativity had found to belong to coordinate systems. Because coordinate systems, being pure fictions, were all precisely equivalent so far as objective validity was concerned, the cosmological presumption declared that *all aspects of the universe* must be precisely equivalent also. It is like saying that because all languages are equally valid for stating the propositions of Euclid, therefore all the propositions of Euclid must be equivalent to one another. The essential distinction between the objective world on the one hand and the rational observer of it on the other, which, I repeat, has always been fundamental in science and which relativity restored and reformulated, is here again destroyed, and a purely imaginary characteristic is foisted on the universe and presented as a basic axiom of science to which general relativity must yield place.

The next step is obvious. The appearance of the universe must remain the same. But observation seems to show that the universe is scattering apart. Therefore matter must be in process of creation all the time, at such a rate as to compensate for that which recedes from observation. This consequence of the presumption has proved specially attractive, for it has given rise to an alternative scheme in which it is itself taken as a fundamental postulate, without even the support of the baseless cosmological presumption. In this variant of the "new cosmology," as it has been called, and as I will for convenience call it here since it is not literally inaccurate, it is simply asserted as a primary axiom that matter is continually being created (*1*). Something akin to the cosmological presumption then follows by the reverse process of reasoning, for the rate of creation is guessed as being just that at which matter disappears by recession; hence the general aspect of the universe remains the same at all times This is said to conform to the principle of the field equations of general relativity, for you have simply to make a slight change in the tensor on the left-hand side, and the right-hand side will then describe a universe in which creation takes place continually.

I am here again faced with the difficulty that I cannot give a true account of this new cosmology without appearing to ridicule it. I am doing nothing of the kind. I am simply divesting it of the symbolic clothing in which it has been wrapped for formal presentation, and the substance underneath appears ridiculous because it is ridiculous. It is hard for those unacquainted with the mathematics of the subject, and trained in the scientific tradition, to credit that the elementary principles of science are being so openly

outraged as they are here. One naturally inclines to think that the idea of the continual creation of matter has somewhow emerged from mathematical discussion based on scientific observation, and that, whether right or wrong, it is a legitimate inference from what we know. It is nothing of the kind, and it is necessary that that should be clearly understood. It has no other basis than the fancy of a few mathematicians who think how nice it would be if the world were made that way. The mathematics *follows* the fancy, not precedes it; the fancy is credited because it gives scope for mathematical exercise, not because there is any reason to believe it true. Here are the actual words of one of the originators of the scheme (*8*, pp. 95–96):

> Equation (10.5) [Einstein's field equations] may be read either from left to right, showing how the presence of matter affects the geometry, or from right to left, showing how the density, momentum and energy of matter must satisfy the well known conservation laws owing to their relation to geometrical quantities which are automatically conserved.

Remembering what the two sides of the equations mean, we see that what is here asserted is this: we can either observe how the universe behaves and choose a tensor that describes the observed behavior, or we can construct a tensor having whatever properties we like and infer that the universe behaves accordingly. And, in fact, this second alternative is the one chosen in this second form of the new cosmology. A tensor is invented whose divergence does not vanish. This is substituted for the left-hand side of Einstein's field equations, and the divergence of the right-hand side must therefore also not vanish. The right-hand side describes the observable behavior of the universe. Hence the universe must be such that matter is continually being created in it.

How is it, we may well ask, that such a thing is possible after three centuries of scientific progress in which it has over and over again been exemplified that the speculations of even the most gifted of seers go far astray from the reality that observation is to reveal? The question is a psychological one, of course, but it is not out of place to ask it here, for the answer may help us to understand still more clearly what value to place on these chimeras. So far as I can judge, the authors of this new cosmology are primarily concerned about the great difficulty which must face all systems that contemplate a changing universe —namely, how can we conceive it to have begun? They are not content to leave this question unanswered until further knowledge comes; all problems must be solved now. Nor, for some reason, are they content to suppose that at some period in the distant past something happened that does not continually happen now. It seems to them better to suppose that there was no beginning and will be no ending to the material universe, and therefore, tacitly assuming that the universe must conform to their tastes, they declare that this must have been the case.

But if we must really answer all questions immediately, is their solution in fact more intellectually satisfying than that of a special creation? Consider what it implies. Granting for the sake of argument that the nebular red-shift indicates that nebulae are continually receding into inaccessibility by surpassing the speed of light, we are then asked to believe that isolated fundamental particles are continually created within the accessible region in order to maintain the same total quantity of observable matter at all times. One immediately looks for a connection between these two processes. We are led, for instance, to suppose that when a nebula reaches the speed of light its particles might undergo a number of gigantic quantum jumps back into the observable region, something like those pictured in Bohr's original theory of spectra. But this appears impossible, for a nebula, on reaching the speed of light for us, is still observable from another nebula not so far distant in the same direction, and the process would have been observable there before becoming knowable to us. Hence the sudden vanishing of nebulae moving at relatively slow speeds should be observable from one nebula, and therefore from all. No such phenomenon, however, is postulated in the scheme. It seems, therefore, that the recession of nebulae into unobservability and the creation of particles relatively near at hand must be independent processes. Yet they occur at exactly the same rate —not approximately, but exactly, for the universe must appear the same eternally—eternally in the past and eternally in the future. Such an extreme example of pre-established harmony—if the new cosmologists will forgive the phrase for what they will perhaps regard as an accident—is, to my mind at least, harder to credit than a special creation in the past. If I must choose I choose the latter as the less revolting to common sense, but on the whole I prefer that wise, ingenious and modest sentence, "I do not know."

I cannot help wondering what would happen if the new cosmologists turned their attention to biology, for here there is a very similar problem. We observe living matter and dead matter, but we never experience the creation of living matter out of dead matter alone. We seem here to be faced with the same two possibilities as in cosmology: we can suppose either that, at some epoch in the past, dead matter somehow became alive by a process not yet known, or that the process has been going on eternally, but at such a rate that we cannot detect it. Since the former alternative is "against the spirit of scientific inquiry," the new cosmologists would seem forced to postulate the continual animation of matter. But their own cosmological scheme rules this out. The matter of which the earth is composed is supposed to have been created as separate atoms, and therefore could not have begun to live for many millions of years. Hence terrestrial life, if it is admitted to exist, must have begun at some particular epoch. There seems to be no escape from the difficulty except by denying the existence of living matter or by violating "the spirit of scientific inquiry." I fear that the excessive inbreeding, which is the curse of mathematical physics, is responsible for the oversight of this problem.

But if inability to see science as a whole blinds the authors to the difficulties in their scheme, I think it is lack of historical perspective that allows it to be invested with the false charm by which they are fascinated. In every age there is a certain climate of opinion that predisposes thinkers toward a certain type of view and makes it very difficult for them to resist arguments that conform to it. This could be illustrated by examples from any period in history. I will merely mention the philosophy, of which I myself am old enough to remember the later stages, which sought to explain everything in terms of matter and energy; Haeckel's *Riddle of the Universe* was a typical representative of this view. There was a fundamental basis of *substance*: this was neither creatable nor destructible, so all phenomena must be caused by its change from one form to another. Life, therefore, was simply one of the forms assumed by this substance, which in another form was kinetic energy and in another, electromagnetic strain. Hence freedom of the will was an illusion, all actions of living creatures, even the highest, being as rigidly determined as the motions of the planets.

Today this way of thinking makes little appeal; our prejudices are different. Now it is the exceptional that is out of favor. By a sort of cosmic democracy we are predisposed to deny any unique characteristic to anything, and whatever we happen to see now, all the universe must see at all times. To think otherwise is to be pre-Copernican. And just as universal determinism could point to the triumphs of Newtonian mechanics as demanding its dogmas, so this new view can point to general relativity, with its equivalence of status for all coordinate systems.

In these circumstances the voice of science is now as it has always been: "Never mind what seems the proper thing to believe; what is the *evidence* for what you say?" And the answer which the new cosmologists must give is just that which was available to the universal determinists, namely, "None at all." When you ask for the evidence that the movements of living creatures are determined, the first thing to decide is, what is a living creature? And the only answer we can give is that it is a piece of matter whose movements are not determined. Subject a mouse to given forces and calculate how it will move. If it moves that way, it is dead; if it does not, it is alive. That is the only way we know of distinguishing between the two cases. Examined scientifically, therefore, the 19th century argument ran thus: the movements of some bodies are determined, the evidence being that we have determined them; therefore the movements of other bodies are determined, although the evidence shows that they are not.

The cosmological presumption is in similar case. The great consequences that have followed our full recognition of the equivalence of coordinate systems predispose us to assert the equivalence of events at all places and times in the universe, but the evidence, such as it is, is all against it. Every process we know, on the small or the large scale, is a one-way process, showing a preference for one direction over the opposite. The system of nebulae expands and does not contract, gravitation is an attraction and not a repulsion, the entropy of a closed system increases and does not decrease, every chemical process tends toward a state of equilibrium from which the substances concerned do not of themselves depart, organic evolution proceeds in one direction and not the opposite; and so on. There is nothing whatever in nature that indicates that any course of events is reversible. Admittedly the evidence is small compared with the magnitude of the problem, but it is all we have. I accept the spirit of the cosmological presumption to this extent, that I do not believe that the universe conforms to the prejudices of the 20th century rather than to those of the 19th century. We shall be better employed in following the scientific program than in listening to either.

I hope that I need not say that none of the considerations I have put before you should tend in the smallest degree to diminish the importance that belongs to imagination in science, when the word is used in its true sense as referring to the ability to form vivid images of possible happenings, and not in the sense in which I have had sometimes to use it, as indicating the invention of arbitrary postulates. No great scientific work has been done without the free and bold exercise of this indispensable quality, and science will stop if ever it is treated as a branch of logic as surely as if it is given over to uncontrolled fancy. By all means keep imagination free, but let it be directed, and let its products be examined and properly assessed before they are announced as discoveries of the order of nature. Even idle speculation may not be quite valueless if it is recognized for what it is. If the new cosmologists would observe this proviso, calling a spade a spade and not a perfect agricultural principle, one's only cause for regret would be that such great talents were spent for so little profit. That would indeed be a sufficient calamity, but I am not without hope for the future. I am not yet convinced that facility in performing mathematical operations must inevitably deprive its possessor of the power of elementary reasoning, though I admit that the evidence against me is strong. But I have a suggestion to offer. Let our younger cosmologists forget cosmology for a space of 3 years—the universe is patient, it can wait—and instead read the history of science. I do not mean books about the history of science, though they are infinitely better than nothing, but the works of the great scientists themselves; let them read Gilbert, Galileo, Harvey, Newton, Boyle, Black, Lavoisier, Faraday, Darwin, Huxley, Pasteur, Kirchhoff, Rutherford. Then let them spend 6 months thinking over what they have read and asking themselves what meaning it has for the work of today. And after that let them return to cosmology and give their attention again to the great problems into which they have prematurely rushed. I am convinced that if they would have the courage and the greatness of mind to do this, astronomy would not only be enor-

mously the richer, but they themselves would thank me sincerely for the advice.

I know it is difficult. I can conceive vividly enough that when one's mathematical facility greatly exceeds his judgment of scientific authenticity, the temptation to exercise it indiscriminately on any premises that will give it scope must be overwhelming; and I am too sensible of my own good fortune in being free from that temptation to enjoy the task of arraigning its victims. But it is a task which I cannot escape. One who, however unworthy, accepts the honor of presiding over one of the foremost scientific societies of the world, accepts at the same time a responsibility. The ideas to which we give publicity do not remain our private possession; they are accepted as genuine scientific pronouncements, and as such influence the thinking of philosophers, theologians, and all who realize that in no intellectual problem, however fundamental, can scientific research now be ignored. And so, when it happens that we have published, in the name of science, so-called "principles" that, in origin and character, are identical with the "principles" that all celestial movements are circular and all celestial bodies immutable, it becomes my duty to point out that this is precisely the kind of cerebration that science was created to displace. And the responsibility is not only mine; it rests on all of us. In cosmology we are again, like the philosophers of the Middle Ages, facing a world almost entirely unknown. We need to cultivate the restraint of Galileo, who left the world of angels and spirits until the time should come when it could be explored, and contented himself with such principles as he could extract with confidence from experience, though the resolution committed him to such trivialities as the timing of balls rolling down grooves. It is that self-control—the voluntary restriction to the task of extending knowledge outward from the observed to the unobserved instead of imposing imagined universal principles inward on the world of observation—that is the essential hallmark of the man of science, distinguishing him most fundamentally from the nonscientific philosopher. It is the duty of all of us, now that the old temptation is with us again, to see to it that we preserve the spirit of true scientific inquiry which, as a scientific society, we are pledged to honor.

References and Notes

* Presidential address in 1953 before the Royal Astronomical Society (London); reprinted by permission from the *Monthly Notices of the Royal Astronomical Society* **113**, 393 (1953).

1. *Monthly Notices Roy. Astron. Soc.* **108**, 372 (1948).
2. Galileo, *Dialogues on the Two Chief Systems of the World, Fourth Day* (Salusbury's translation), p. 407.
3. I. Newton, *Opera* (Horsley's ed.), iv, p. 437.
4. ———, *Opticks*, ed. 4, p. 377.
5. *Monthly Notices Roy. Astron. Soc.* **108**, 104 (1948).
6. *Ibid.*, **108**, 252 (1948).
7. This is accepted so implicitly by psychologists that it is difficult to find an actual statement of it. R. W. Russell, however, has kindly drawn my attention to the following two passages; "The term *repetition*, as applied to an event of nature, is clearly ideal and nonrealizable in any concrete case, like the term *equality* and the term *circle*. No two concrete objects in nature ever ideally equal in every respect. No two experiments on any event of nature were ever ideal repetitions, because nature never repeats herself. [G. Humphrey, *Thinking* (Methuen, 1951), p. 113.] This quite general statement is then shown to have practical importance in particular psychological experiments. The second passage is as follows: "These two constant errors, practice effects and fatigue effects, *are potentially constant errors in almost all psychological experiments, hence, become factors which must be considered in the design of all experiments.*" [B. J. Underwood, *Experimental Psychology* (Appleton-Century-Crofts, 1949), p. 29.] The terms *practice effects* and *fatigue effects* are self-explanatory; *constant* error has a technical meaning.
8. H. Bondi, *Cosmology* (Cambridge Univ. Press, 1952), p. 11.
9. In other connections the distinction between these alternatives is important, but not here. The essential requirement in this address is to distinguish between that uncontrollable something which we investigate, whatever it may be called, and the mental machinery of investigation, which we can choose freely.
10. E. A. Milne, *Modern Cosmology and the Christian Idea of God* (Oxford Univ. Press, 1952).
11. A. S. Eddington, *Relativity Theory of Protons and Electrons* (1936), sec. 16.95: E. A. Milne, *op. cit.*, p. 33.
12. A. S. Eddington, *The Philosophy of Physical Science* (1939), chap. VIII.
13. Eddington chose, among other things, certain "forms of thought," which in fact are not necessary but have usually been assumed in empirical science (*12*).
14. See, for example, E. A. Milne, *Relativity, Gravitation and World Structure* (Oxford Univ. Press, 1935) and numerous other writings.
15. ———, *Ibid.*, p. 16: "Relativity and solipsism are incompatibles. Relativity is the complete denial of the solipsist position." This is said of "relativity" and "kinematical relativity," as the context makes clear. Actually, of course, relativity has nothing whatever to do with solipsism; if there is only a single observer he can choose his coordinate system as he wishes, and if there are 10^{70} of them, they can do no more. But the passage indicates clearly enough how the purely conceptual coordinate system of relativity had become transformed into a sentient being.
16. *Nature*, **155**, 512 (1945), and, by implication, H. Bondi, *Cosmology* (Cambridge Univ. Press, 1952), p. 123.

If you have had your attention directed to the novelties in thought in your own lifetime, you will have observed that almost all really new ideas have a certain aspect of foolishness when they are first produced, and almost any idea which jogs you out of your current abstractions may be better than nothing.—A. N. WHITEHEAD.

News and Notes

Religion in the Age of Science

Ten scientists explained how they thought scientific and religious knowledge could be integrated at the Conference on Religion in the Age of Science that took place from 31 July to 6 Aug. at Star Island in the Isles of Shoals [*Science* 119, 683 (1954)]. Conference members, numbering more than 200, included theologians, clergymen, and laymen of ten Protesant denominations and Jewish, Buddhist, and Vedanta groups. Since it would be impossible to do justice to the whole conference here, and since it is expected that a volume of its proceedings will be published, the following paragraphs simply represent one man's feeling about some of the significant elements.

Called in the faith that the understanding of religious "truth and reality" is related to the understanding of scientific "truth and reality," the conference stimulated considerable discussion concerning the nature of scientific "truth," particularly following the papers of Philipp Frank (Harvard) and Henry Margenau (Yale). And it was perhaps an occasion of historic significance when, in a later discussion, members of the clergy, in setting forth a certain concept of God, were challenged by other members of the clergy concerning their evidence in the "P plane" (directly observed phenomena, à la Margenau) for substantiating that concept. While there were a number of both scientists and clergymen who held that religious truth was hardly susceptible to being approached by scientific methods, except perhaps in the negative sense of being prohibited by scientific beliefs, there was a strong and seemingly growing recognition that today man can increase the scope and validity of his understanding of his destiny and of his relationship to that "in which he lives and moves and has his being," not only by reading ancient texts, but also by building up the science of theology in harmony with other science. Here the scientists were referring to pure science in search of truth about cosmos and man, and they warned against confusing it with technological applications.

Edwin Kemble (Harvard), although saying that he opposed the common view "that because science and religion operated on different planes they are independent of one another," nevertheless felt that "to those who envisage religion as the life of God in the soul of man . . . a religion that accepts the philosophy of materialism is no religion at all"; and he went on to point out the limitations of the "orthodoxy" of scientific materialism and the necessity for the use of intuitive judgment and of faith in human action. Margenau and others pointed out that "intuition" operated in the creative work of the scientist; and there was considerable agreement, though not by any means complete, that the use of intuition was a common rather than a differentiating factor between the operations of scientists and the formulators of religious truth.

C. J. Ducasse (Brown), after reviewing the disastrous impact of modern scientific conceptions of the world on medieval religious concepts, suggested that "what science has undermined is perhaps not religion itself, but only some of the dogmas of the orthodox forms. . . ." He did not distinguish religious beliefs from scientific beliefs on the basis of any difference in content or subject matter, but rather on the basis of the function of religious beliefs to provide guidance for satisfactory behavior of the individual for himself and for society; and he suggested that religious beliefs might eventually become scientific in that they might be formulated in such a way as to be demonstrably true in the face of the facts. Samuel H. Miller (Andover Newton Theological School) said that the "religious faith of western civilization no longer constitutes the unifying strength or center of perspective." The coming religion will not be "elicited by magic abracadabra, but [will be] one implicit in the nature of things."

That the attempt to integrate religious doctrines with those of science would tend to bring harmony out of the confusion and antipathy among the many religious groups of the world was the theme of one panel discussion. It was pointed out by the historians at the conference that not only does Christendom face the age-old problems of schisms and splits within, but also that we must now take into serious account the great religions of the East and Communism and other semireligious or religious movements that have had a growing appeal in the heart countries of Christian Europe.

Edwin P. Booth (Boston University), indicated his belief that current attempts to bring about unity of the Christian world by the limited ecumenical councils were insufficient in view of the fact that no religion can properly exclude any man, to say nothing of excluding the bulk of the population of the world, which is now physically one world. He thought that ecumenical councils sometimes meet not so much to learn and to yield to a common truth but rather to create a sense of security against the threats of secularism or opposing religious groups. But, Booth felt, real progress toward the goal of one world, a growing clarity and common understanding in religious views like that in the physical sciences, could be developed, if theologians held with integrity to the rationality that characterized some of the great theologians of a half century ago, and if they took as their standard of validation not an ancient text but the experience of living man. He pointed out how past advance in religious thought was of this character. The panel, which included representatives of Hinduism and Buddhism as well as representatives of various Protestant denominations, were confident that the various religious groups at base had much in common, and Swami Akhilananda of the Vedanta Center in Boston expressed his conviction that scientific understanding

would lead to gradual discarding of irrelevant concepts and to concurrence on basic truths. A majority of those at the conference, however, did not seem to want to or expect a single world religious body or a set of doctrines for all men.

The nature of God was the theme of several discussions, stimulated particularly by papers by Harlow Shapley (formerly director of the Harvard Astronomical Observatory) on "The cosmos," by George Wald (Harvard) on "The origin of life," by A. G. Huntsman (University of Toronto) on "This life," by Paul E. Sabine on "Religion and/or science." There was a line of cleavage of opinion about equating God with nature that seemed not so much to separate the clergy from the scientists as to cut between members of both groups. There were a number of attempts to clarify the relationship of God and nature, and A. G. Huntsman said "If God is the spirit or ruler of the universe, we are indeed His creation, and in Him 'we live and move and have our being.' In scientific terms, we are the product of our environment, and we are wholly dependent upon it for all that we are and do." In a humble and honest way, he went on to approach a number of theological questions in the light of science. On this basis he developed the reasons for the validity of the rule that in order to find life man must first seek God's will and obey it. Among the members of the conference there developed jovial references to the scientists as fellow theologians and to theologians as fellow scientists.

Some light from science was shed on certain problems of value in the ordering of human individual life and social life—problems of morality and ethics. The paper by H. B. Phillips (formerly at M.I.T.) on "Freedom and probability," for instance, indicated that individual liberty should be maximized if a society is to receive maximum contributions from individuals. His argument applied the same probability considerations to human, social, or cultural development that Wald had applied to the development of complex organic molecules and living cells from simpler molecules in his account of the origin of life on earth. Phillips pointed out that we do not have prior knowledge of the outcome, and hence of the rightness or wrongness, of the possible acts open to us. If society limits the trials open to its individual members, it reduces the probability that a (right or good) solution will be found. Therefore, the most fruitful form of society is one that allows the maximum liberty to its individual members within the limits set by the requirements for maintaining social structure. This is akin to the requirement of genetic variations for the evolution of biological forms.

Roy G. Hoskins (formerly at Harvard Medical School) pointed out that, among the emergent developments of the biological law of "thou shalt survive," there was a human characteristic called "empathy" that related the individual to the needs of the other members of the society of which he was a part and that motivated him to action that was useful for the group even at times when it was sacrificial of himself. The rule of brotherly love has its basis in cosmic nature.

B. F. Skinner (Harvard) said that experimental psychology was making clear the superiority of a system of rewards over a system of punishments in creating desired new patterns of behavior and that psychology therefore offered powerful tools for ethical training. Some felt that this implied psychological substantiation for the religious insistence on tolerance and love in dealing with other men rather than the aversive methods of punishment or war. Skinner warned that these more powerful and pleasant methods of control could be dangerous when used by clever but unscrupulous men, and he recommended the establishment of suitable social safeguards.

Many of the conferees, scientists as well as clergymen, expressed their fear of the concepts of material causation that are implied in the scientific account of human origins and of the control of human behavior. They were bothered in particular by the apparent denial of those aspects of life that we have traditionally called spirit, purpose, free will, self-determination, and individual responsibility for moral behavior. That human life should be completely bound up in a chain of mechanical cause-and-effect seemed abhorrent. Yet it appeared doubtful to most of the scientists and clergymen that one could find escape from cause-and-effect in science, and there was a tendency to seek escape into some nonscientific realm. But Karl Deutsch (M.I.T.) had pointed out in the opening address that it was neither safe nor dignified for the custodians of religious truth to maintain that their truth lay in the area where science had no answers, that God was a three-letter word for what we do not know; for, time and again in history, science has come up with explanations of what yesterday it could not explain, and religion has had to beat an undignified retreat as its claims for authority where science was ignorant have been successively demolished.

The new strategy, suggested by many from both the camp of science and the camp of religion in this peace conference in the cold war between science and religion, is that theology should no longer stake its claims only in the area where science is ignorant, but rather that theology should accept and integrate with the developments of the several branches of knowledge represented by the sciences.

But this new strategy means a face-to-face encounter with the apparent threats to the reality of spirit, free will, responsibility, purpose, and so on. One theologian, Bradshaw of Bangor Theological Seminary, implied that perhaps the trouble was more apparent than real, perhaps it was largely a verbal confusion, when he suggested that his colleagues state clearly for the scientists what they mean by the word "spirit" and what particular phenomena of experience they refer to by the term "spiritual" in order to get the scientists' account of these same phenomena. Skinner, when questioned about free will, referred to the doctrines of John Calvin—with the implication that a strict determinism was neither new nor antagonistic to

a vital religious doctrine. Wald pointed out that acceptance of the scientific account of man in terms of molecules takes nothing whatever away from his full reality. Hoskins, as well as Wald, mentioned the emergence of new levels of being or structure that were made up of patterns of the "bricks" of a lower level. The scientific explanation of lower levels does not deny any reality to the higher levels. Although the building material of man may be the lowly molecule, this building material has been shaped into the most complex known organism, with its "spiritual" values.

Ducasse said it was an error to suppose that causation is necessarily compulsion. When, for instance, hunger causes a man "to eat and he *likes* the food he eats, he is then correctly said to be eating not under compulsion but freely. Causality and freedom thus are not inherently incompatible . . . for the alternative to causality is pure objective chance, and volitions that were not determined by motives but were matters of pure chance would be not free but irresponsible. The alternative to freedom is compulsion, not determinism." Compulsion he defined as that class of caused behavior which the subject happens to dislike, "for instance, handing a purse to a hold-up man."

Gerald Holton (Harvard) discussed the religious motivations of some of the great scientists. Frank, in his paper, said that ethical and religious requirements do enter into the constitution of scientific theories, even in physics. In a later comment, he said that the whole structure of knowledge inevitably had to be interconnected, with social, ethical, and religious concepts bound into physical theory and vice versa. He pointed out that one may temporarily restrict the phenomena to be accounted for by a theoretical system in order to achieve certain partial success in understanding; but ultimately no phenomenon can be disregarded, and no discontinuity between partially isolated systems of theoretical explanation can be satisfactory.

The general tone of the conference throughout was one of cooperative cordiality and even elation. There were many testimonials of exciting mind-stretching and new appreciations on the part of both the scientists and the clergy. Booth's daily chapel essays provided many insights into the values and validity of some of the great religious traditions of the world. The clergy and lay members of the conference were deeply impressed with the grand sweep of knowledge about man and his destiny in terms of the scientific view of the universe; and they were amazed at the concern of scientists to help, as Wald put it, to "organize human experience so that persons can feel at home in the universe, some sense of direction in their daily lives, some hope for the future, some purpose in their lives."

Many came away with a deeper understanding of what Pope Pius XII may have meant when he said to the Pontifical Academy of Science, 22 Nov. 1951: "In fact, according to the measure of its progress, and contrary to affirmations advanced in the past, true science discovers God in an ever-increasing degree—as though God were waiting behind every door opened by science." There were suggestions that more specific cooperation should be developed between scientists and theologians. It was proposed that interdisciplinary seminars be established to develop modern moral and religious doctrine in the light of science and that all relevant branches of science should be represented in theological school faculties. There was widespread confidence that the conference had opened a way to an integration of religion and science that would indeed provide a more hopeful basis for cooperative and satisfactory living on the part of men in the age of science. Perhaps, as Shapley suggested, modern science and religion are identical.

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International Congress of Psychology

At the 13th International Congress of Psychology which was held in Stockholm in July 1951, the executive committee of the newly formed International Union of Scientific Psychology, realizing that it would be less difficult to obtain visas for Canada than for the United States, accepted the generous invitation of the Canadian Psychological Association to hold the 14th congress in Canada, with the American Psychological Association as joint sponsor. The congress took place at McGill University and at the University of Montreal, 7–12 June. The total membership consisted of 1021 persons from 31 countries. A substantial sum of money from private individuals and scientific bodies provided financial assistance for many of the foreign visitors who attended the meetings.

Six psychologists came from the U.S.S.R. The program committee was informed late in the spring that these men would like to present papers, but it was possible to arrange for only five of them on the program. They brought with them 2000 copies of a booklet, *Communications at the XIV International Congress of Psychology*, that contained six papers in Russian and translations in both English and French. This booklet was distributed to the members of the congress.

Previous international congresses had accepted practically all papers submitted, and this resulted in programs that were primarily a series of disconnected reports. Dissatisfaction with these congresses led the International Union to suggest that a different procedure be followed by the 14th congress, a concrete hint for more symposiums. The program committee decided on an entirely new venture—a program consisting of symposiums and groups of papers about a common theme, and a few evening lectures. There were only three to five papers and a few discussants in each session; participation was by invitation only, and selection was made on the basis of active research, with the choice in favor of the younger members of the profession. The topics for the sessions were selected with great care; for example, the clinical papers

stressed the scientific implications of clinical data rather than the improvement of clinical procedures, the papers on industrial psychology dealt with the interaction of social and technologic factors in the industrial field, and the papers on individual differences were related to national policy. The congress was considered a great success, chiefly because of the program, which may well serve as a model for the future.

The copresidents of the congress, E. A. Bott of the University of Toronto and E. C. Tolman of the University of California, spoke, respectively, on "The influence of organization on psychology as a science" and "Performance vectors and the unconscious." Tolman took occasion to criticize the "rising tide of anti-intellectualism" on both sides of the iron and of the bamboo curtains, caused by the people of the various countries being dominated by fear. Other evening lectures were "Perceptual and cognitive structures in the development of the infant's idea of space" by J. Piaget of the University of Geneva and the University of Paris; "Perception and cognition" by Albert Michotte van den Berck of the University of Louvain; and "The cortical record of the stream of consciousness" by Wilder G. Penfield of McGill University. The results presented by Penfield are of great significance. He has found that electric stimulation of the temporal cortex of the brain causes patients to review the sights, sounds, and thoughts of previous experiences. These recalls are clearer and more detailed than most memories under normal conditions. The implications of these findings are intriguing in regard to the functions of the cortex.

A list of the symposiums will give the best idea of psychology today as it appeared at the congress: "Consciousness: revised and revived"; "Projective techniques and psychological theory"; "Multivariate methods for electronic computers"; "The relation of the person to his environment"; "Recent developments in sensory psychology"; "Present status of Freudian theory"; "Social variables in personality determination"; "Mechanisms of motivated behavior"; "Some new approaches to the multidimensional analysis of behavior"; "Social and technological organization in industrial production systems"; "Recent trends in perceptual theory"; "Motor skills"; "Personal contact and change in intergroup attitudes"; "European characterology"; "Information theory in psychology"; "Cross-national research in social psychology"; "Problem-solving behavior"; "Recent developments in color."

Four other symposiums especially exemplify the scope and interest of the program. The papers given in these symposiums follow. In the symposium "Experimental and theoretical analysis of instinctive behavior," the results of recent research on instinctive behavior in animals were reported and the theories were discussed. G. P. Baerends (Groningen) spoke on "Egg recognition in the herring gull"; J. J. A. van Iersel (Leyden) spoke on "An attempt at an analysis of parental behavior of the male three-spined stickleback"; D. S. Lehrman (Rutgers) spoke on "Maternal behavior in birds and the problem of instincts"; and W. S. Verplanck (Harvard), although absent, submitted a paper on "Learned and innate behavior: toward a *rapprochement* of concepts."

S. Biesheuvel (Johannesburg) presented a paper on the general problem of the symposium, "Individual differences in abilities and their implication for national policy," in relation to South Africa. V. Coucheron Jarl (Oslo) had a paper on "Intellectual abilities and schooling as a psychological and social issue." M. Yela (Madrid) spoke on "Some historical and experimental remarks on selection problems in Spain"; and A. H. El Koussy (Cairo) spoke on "Individual differences and social reconstruction."

In the session on "Recent advances in conditioning," J. Lacey (Fels Research Institute) had a paper on "Conditioned autonomic responses in the experimental study of anxiety"; H. Eysenck (London) on "Conditioning and personality"; A. Leontiev (Moscow) on "Formation and nature of mental properties and processes of man"; B. M. Teplov (Moscow) on "Theory of the types of activity of the central nervous system and psychology"; E. A. Asparatjan (Moscow) on "Switching of conditioned-reflex activity as a special form of its changeability"; E. N. Sokolov (Moscow) on "Higher nervous activity and the problem of perception." It was of interest to those present to learn that the Russian psychologists had returned to the conditioned-reflex theory of Pavlov.

In the symposium on "Cerebral functions and behavior," findings from a variety of approaches (electroencephalographic, experimental-surgical, therapeuto-surgical, and clinical) and their theoretical implications were considered. H. H. Jasper (McGill) gave a paper on "Correlates between psychological processes and the electrical activity of the brain"; H. E. Rosvold and J. M. R. Delgado (Yale) on "Effects on behavior of electrical stimulation of the monkey's brain"; A. L. Benton (Iowa State) on "Right-left identification, finger localization, and cerebral status"; and A. Petrie (London) on "Effects on personality of excisions in different regions of the brain."

Two special convocations were held at which McGill University gave honorary degrees to E. A. Bott, Jean Piaget, and E. C. Tolman, and the University of Montreal gave honorary degrees to A. Michotte van den Berck, Henri Piéron, and H. S. Langfeld.

At the meeting of the assembly of the International Union of Scientific Psychology, J. Piaget was elected president of the union and E. T. Rasmussen was elected vice president. The other members of the assembly who were elected are O. Klineberg, Frederic Bartlett, H. Piéron, N. Mailloux, J. Drever, H. S. Langfeld, A. Michotte van den Berck, J. Elmgren, H. C. J. Duyker, and J. Germain. The executive committee elected O. Klineberg secretary general and N. Mailloux treasurer to take office 1 Jan. 1955. The next congress is scheduled for 1957.

H. S. Langfeld, *Secretary General*
International Union of Scientific Psychology
Eno Hall, Princeton University

Science News

Recently in *Science* [119, 676 (1954)] attention was drawn to a proposal by E. Finlay-Freundlich that light suffers an attenuation of energy, resulting in a **red-shift,** whenever it passes through an energy-containing enclosure, and a formula was presented for this red-shift, based upon diverse observational phenomena in astronomy. In a letter to the editor in the *Physical Review* for 1 Oct., H. L. Helfer has demonstrated that the exhibited formula is not universally applicable and that, considering the nature of the objections raised, severe modifications of the hypothesis are necessitated if it is not to be completely abandoned.

The first objection raised is based upon a consideration of systems of double stars whose orbital planes enclose the line of sight. In such systems, the total path-length along which light travels from either star to us varies as the stars revolve about each other. Since the light path of any one star lies in the radiation field of the other star, the resulting situation is that of light transversing an energy-containing enclosure of periodically varying length, which should result in a periodic red-shift according to the hypothesis. Calculations for five double-star systems that are well observed predict a red-shift several orders of magnitude too large to be reconciled with the observations. For one system in particular for which the observational material is very good, the predicted shift is of the order of 200,000 sec.$^{-1}$

Helfer also points out the existence of a group of binary stars in which it is actually possible to observe, during eclipse, the light of one star shining *through* the other. This results in an enclosure of very high energy density, and again the predicted red-shift is several orders of magnitude too large. The latter objection is to be given less weight, owing to the difficulties of interpreting the observational phenomena. The author feels that these two arguments eliminate the possibility of any photon-photon or photon-matter interactions resulting in a red-shift of anywhere near the order of magnitude predicted.

Two French cave explorers, Jean Cadoux and Georges Garby, have reported that they descended 2485 ft in the Berger cave near Grenoble, France, for a world **record for depth** in a natural cavern.

A long-sought instrument, a **millimeter wave generator** that produces electromagnetic waves in the region between microwaves and infrared rays, has been devised by Hans Motz of Stanford University's Microwave Laboratory. The apparatus has generated frequencies up to 2×10^6 Mcy/sec (wavelength, 0.15 mm) at powers from 10 to 100 mw. Because of this they can be used to "see" into the atom, revealing events that take place less than 10^{-6} μsec apart. The instrument's main components are a 3-ft, 2-Mev linear accelerator, an "undulator," and an "echelette spectrometer." The accelerator produces a pulsed beam of electrons that is fed into a silver waveguide in the undulator with a speed slightly less than the speed of light. Eight separate, alternating magnetic fields in the undulator act on the electrons, causing them to oscillate. The waves then pass into the echelette spectrometer, which consists of a 2-ft parabolic mirror facing a grooved aluminum plate. The spectrometer removes the undesired frequencies.

J. Robert Oppenheimer's 26 colleagues at the Institute for Advanced Study in Princeton issued the following statement on 1 July.

Now that the official decision concerning the question of Dr. Oppenheimer's security clearance has been rendered, the undersigned permanent members and professors emeriti of the Institute for Advanced Study consider that in all propriety they may publicly express their feelings concerning Dr. Oppenheimer in the light of the charges brought against him.

We, who have known him as a colleague, as director of our own institution, and as a neighbor in a small and intimate community, had from the first complete confidence in his loyalty to the United States, his discretion in guarding its secrets, and his deep concern for its safety, strength, and welfare. Our confidence in his loyalty and patriotic devotion remains unimpaired as our admiration for his magnificent public service is undiminished.

Dr. Oppenheimer has performed for this country service of another kind, more indirect and less conspicuous but nevertheless, we believe, of great significance. For seven years now he has with inspired devotion directed the work of the Institute for Advanced Study, for which he has proved himself singularly well-suited by the unique combination of his personality, his broad scientific interests, and his acute scholarship. We are proud to give public expression at this time to our loyal appreciation of the many benefits that we all derive from our association with him in this capacity.

J. Lenihan, leader of a research team in the department of surgery at Glasgow University that is investigating the causes of peptic ulcer, has devised a **Geiger counter** which is small enough to attach to a gastroscope. Development of this apparatus makes it possible to interpret exactly what is going on in the stomach after use of radioactive isotopes.—K. L. H.

Kirtley F. Mather, in the announcement of his retirement after more than a decade of service as book review editor of the *American Scientist* has some pertinent things to say about the **art of book reviewing.** [42, 506 (July 1954)]

. . . There are few, if any, objective standards by which all books may be appraised. Beyond the basic principles of grammatical construction and typographical composition, each author may draft his own ground rules. Rugged individualism occupies one of its few remaining strongholds, and opportunity is still afforded for diversified personal expression. No book reviewer should imagine for a moment that he represents anybody but himself when proceeding with his task.

It was necessary therefore for me to establish some sort of pattern for "The Scientist's Bookshelf," early in the game. It seemed to me that its primary function was informational, rather than pontifical. Its readers would

probably be more concerned with knowing what a book is about, than what I think about a book. At the same time, it was necessary for me to accept judicial responsibility and express opinions concerning the merits of the books considered for review. This involves scientific accuracy, cogency of generalizations and conclusions, depth and breadth of comprehension, as well as clarity of presentation, readability, and probable success as an educational tool. Some of the books that have passed across my desk, since the spring of 1943, have been worthy, in my opinion, of high praise, and that I have tried to indicate in one way or another. Many could best be "damned with faint praise." Others, for one reason or another, have been merely listed or omitted entirely—mostly for lack of space. There have been a very few books, however, which I felt should be harshly criticized, and occasionally I have expressed adverse opinions. Incidentally, I have found that only a few minutes are required to get sufficient knowledge of a book to write a noncommital or faintly commendatory review of it, whereas it takes hours of careful reading to prepare oneself for writing either a strongly favorable or a sharply critical review.

. . . The stream of books flowing from the publishing houses between 1942 and 1947 was not so great but that room could be found for review or brief mention of almost the entire output. Since 1947, the flood gates have opened. Even with additional pages at my disposal . . . it has been impossible to maintain the nearly complete coverage of earlier years. Sometimes I wonder whether certain publishers are not more concerned with quantity than with quality, as they make their decisions from day to day. By and large, the publishers who group their books in series, with eminent scientists retained as editors, seem to be most successful, so far as quality of output is concerned. . . .

Marlow W. Olsen and Stanley J. Marsden, elsewhere in this issue, report finding **parthenogenetic development** in the infertile eggs of nonmated and virgin Beltsville Small White turkeys. This type of development, a common method of reproduction in some of the lower forms such as bees and aphids, is of rare occurrence in warm-blooded animals. Current methods of determining fertility in the hatchery may have to be revised.

John H. Lawrence of the University of California has reported to the Joint Congressional Committee on Atomic Energy that a combination of 80 percent **xenon** and 20 percent oxygen has been used an effective nonexplosive anesthetic during major surgery. The heavy gas xenon, companion of helium, neon, argon, and krypton and the rarest element in the atmosphere, heretofore has been supposed to be inert. The discovery came accidentally during experiments on the medicinal effects of various isotopes at the University of Iowa's Laboratory of Medicinal Physics. Doctors observed that subjects exposed to xenon became drowsy. This led to experiments with rats and mice which showed that they could be anesthetized.

Discovery of a powerful **heart poison** in a chemical from the streptococcus germ was announced by Aaron Kellner and Theodore Robertson of New York at the World Congress of Cardiology which took place in Washington, D.C., 12–17 Sept. It is believed that this substance, a protein-digesting enzyme called streptococcal proteinase that was isolated in crystalline form in 1950 by S. D. Elliott of the Rockefeller Institute, may be the cause of heart damage following rheumatic fever attacks.

Kellner and Robertson injected the enzyme into the veins of rabbits, mice, guinea pigs, and cats. It caused striking damage to the muscles and valves of the animals' hearts, with an inflammatory reaction. With only a few exceptions, the enzyme's destructive action was confined to the heart. When the enzyme was added to the fluid in which an isolated rat heart was being kept alive and beating, contractions soon decreased in strength and frequency, and heart failure occurred within a few minutes.

Rheumatic fever attacks 30,000 Americans yearly, most of them between the ages of five and 15. It is responsible for an estimated 1,000,000 cases of rheumatic heart disease in the United States.

A specially equipped **car for geotechnical investigations** has recently been added to the rolling stock of the Swedish State Railways. The only one of its kind in Europe and probably in the world, the car is intended for on-the-spot examination of samples from the ground along railway lines, where clay, peat bogs, and so forth present special operational problems.

The results of the **postgraduate alien training program**, begun in mid-1949 under the U.S. Information and Educational Exchange Act of 1948, are reported in the 3 Sept. issue of the *Journal of the American Medical Association* by Harold S. Diehl, Edwin I. Crosby, and Paul K. Kaetzel of the Health Resources Advisory Committee of the Office of Defense Mobilization. A large increase in alien physicians taking postgraduate work in the U.S. has helped fill the gaps left by the many young doctors now on active military duty. The number of aliens on U.S. hospital staffs more than doubled from 1950–51 to 1953–54. During the 1953–54 school year, 5589 foreign physicians held appointments as interns, residents, or fellows on house staffs of the civilian hospitals approved for such training by the Department of State. Three years before the total was 2072.

These aliens cut the number of vacancies in those hospitals down to 20 percent for residents and 30 percent for interns. Without them the percentage would have been "considerably greater," since "many young physicians who would normally be taking postgraduate work are on active military duty." Aliens made up 22 percent of the total house staffs in the approved hospitals. Most of them were located in general hospitals which do not serve as major teaching hospitals for medical schools. They made up almost half the staffs of tuberculosis hospitals approved for alien training, and about one-fourth of the staffs of mental hospitals, but only about one-tenth of the teaching hospital staffs.

Three states—Mississippi, North Dakota, and Arkansas—have no alien physicians in training.

Largely because of state licensing laws, more than two-thirds of them are in five states—New York, Ohio, New Jersey, Illinois, and Massachusetts. In New Jersey, 65 percent of the house-staff positions are filled by aliens, and in New York, Illinois, and Ohio about 30 percent.

Nutritional studies with the **white-throated wood rat** (*Neotoma mexicana*) have been under investigation recently by Robert Van Reen and Paul B. Pearson at the McCollum-Pratt Institute, Johns Hopkins University. According to their forthcoming paper in *Science*, the wood rat is of interest in several respects, particularly in that water intake is large under laboratory conditions. It was not possible, however, to maintain the animals on a purified-type diet, and use of the species for research purposes appears unlikely.

Scientists in the News

On 15 Sept., President Eisenhower "very reluctantly" accepted the resignation of **Henry deWolf Smyth** as a member of the Atomic Energy Commission, effective 30 Sept. The AEC's only scientist member will be succeeded by **Willard F. Libby**, a professor in the University of Chicago's chemistry department and in its Institute for Nuclear Studies, who has been serving as a member of the General Advisory Committee to the AEC.

Author of the famous "Smyth Report" on development of the atomic bomb that was published soon after World War II, Smyth was the only one of the five commissioners who opposed determining that J. Robert Oppenheimer was a security risk. In his letter of resignation Smyth did not mention the Oppenheimer case nor did he mention differences with Lewis L. Strauss. (The scientist told the Joint Congressional Committee on Atomic Energy last July that there had been an air of tension on the commission since Strauss became chairman.) Smyth also opposed the controversial Dixon-Yates contract which the AEC and the Tennessee Valley Authority are negotiating with a private power syndicate. Such a contract would cast the AEC in the role of a "power broker," he asserted. Strauss has defended the proposed contract. Smyth, in his letter to the President, emphasized the atoms-for-peace theme as well as development of atomic weapons. Smyth plans to return to Princeton University, where he had been chairman of the physics department, to accept the newly created post of chairman of the Board of Scientific and Engineering Research.

President Eisenhower now has appointed three commissioners: Strauss, Joseph Campbell, and Libby; Thomas E. Murray is a Truman appointee. There is still one vacancy—created by the expiration on 30 June of the term of Eugene Zuckert.

Oliver S. Reading, chief of the division of photogrammetry in the Coast and Geodetic Survey, U.S. Department of Commerce, retired on 31 Aug. after more than 39 yr of service. He joined the Geodetic Survey in 1915, and made progressive advancement to his present rank of captain. His early field assignments with the bureau included hydrographic and geodetic surveying along the coasts of the United States, Alaska, and the Philippine Islands.

During his long service Reading won international recognition as an expert in photogrammetry. He was a pioneer in this field and played a major part in coordinating aerial photogrammetric procedures with the charting activities of the Survey. Among the various types of inventions for which he has been responsible are the nine-lens aerial camera and its associated equipment, the 50-in. precision copying camera, the projection ruling machine, and the coordinate setting machine. Because of his valuable contributions, Reading was awarded the 1947 Department of Commerce silver medal for meritorious service.

Reading was one of the principal organizers of the American Society of Photogrammetry, which now has more than 3000 members. He served as chairman of the American delegations to the 5th International Congress on Photogrammetry and to the 6th International Congress of Surveyors in Rome during 1938. The State Department appointed him president of the American delegation that attended the meeting of the International Society of Photogrammetry at The Hague, in Sept. 1948; during the meeting he was elected president of the society and presided at the 1952 international meeting held in Washington, D.C. In 1953 Reading visited London, Paris, and Munich, attending meetings of the Congress of International Federation of Surveyors, Council of the International Society of Photogrammetry, and the meeting of the Organization for Photogrammetric Research.

A portrait of **Charles M. B. Cadwalader**, of Fort Washington, Pa., president of the Academy of Natural Sciences of Philadelphia from 1937 to 1951, was accepted by the board of trustees at a meeting of Cadwalader's former associates on 28 Sept.

Kamel el Mallakh, the Egyptologist who discovered Cheops' solar funeral ship in May, will probably visit this country in October.

Cornell University has announced the appointment of **Thomas P. Almy** as director of the Cornell (2nd) division at the New York University–Bellevue Medical Center. He succeeds E. Hugh Luckey, who became dean of Cornell University Medical College on 1 July. Almy, a specialist in gastrointestinal disease, will rank as visiting physician at Bellevue, while continuing as associate professor of medicine at Cornell.

Ronald Richter, Austrian-born scientist who once headed Argentina's atomic energy research, has challenged Argentine experts to disclose the results of the experiments he led so the world could judge whether he was a fraud or a physicist. Goaded by charges made recently in the House of Deputies that his experiments had failed, Richter has asked President Juan Perón

and Congress to hold open hearings on his contribution to Argentine science. He said he had been waiting for a "fair settlement" of his case for more than 18 mo, and added "I would like to declare categorically that there were no scientific errors or negative experimental results in the Huemul project so long as I directed it." Richter's experiments are said to have cost the Argentine Government more than 1,000,000,000 P. (about $70,000,000).

Robert L. Sproull, associate professor of physics at Cornell University, has been appointed editor of the *Journal of Applied Physics,* a monthly publication of the American Institute of Physics.

Berch W. Henry has been appointed head of the new Southern Institute of Forest Genetics that the Southern Forest Experiment Station is establishing at the Harrison Experimental Forest in southern Mississippi. The institute, which is to serve the entire South, will investigate fundamental problems in forest genetics, do applied research in tree selection and breeding, and serve as a demonstration area and source of information for forest genetics research and application.

Shinya Inoué, well known for studies of the ultrastructure of the mitotic spindle, left the staff of Toyko Metropolitan University in September to come to the United States to develop research in experimental cytology and submicroscopic morphology in the department of biology at the University of Rochester. At Rochester he will also participate in an advanced graduate course: "The cell and its organelles."

The University of Notre Dame has announced the appointment of **Arthur L. Schipper,** associate professor of biology, as editor of the *American Midland Naturalist.* He succeeds **John D. Mizelle,** who requested relief so that he might return to a full program of teaching and research; Mizelle, however, will remain on the editorial staff as associate editor for invertebrate zoology.

A distinguished foreign visitor, **Earl J. King,** professor of chemical pathology, Postgraduate School of Medicine, University of London, delivered the seventh Walter Estell Lee lecture on 20 Sept. during the 5th annual institute of the Graduate School of Medicine of the University of Pennsylvania.

William S. Godfrey, Jr., of the Logan Museum, Beloit College, has been appointed executive secretary and secretary of the American Anthropological Association. He will take over the two offices from **Frederick Johnson** of the Peabody Foundation in Andover, Mass., who retired on 15 Sept.

A physics scholarship fund in memory of **Irving Shaknov,** who was killed in action in Korea 2 yr ago while serving with the United States Navy's Operations Evaluation Group, has been announced by the Massachusetts Institute of Technology. Dr. Shaknov's parents were not permitted to disclose even to relatives and friends the news that he was missing. In the period before their son's death was made public, they were obliged to make others believe that they were in close touch with their son at all times. The secrecy surrounding Dr. Shaknov's death was lifted recently in Washington at a ceremony conferring a Medal of Freedom posthumously. At the same time, a portrait of Dr. Shaknov, by Jeff T. Boswell, was unveiled in the headquarters of the Operations Evaluations Group in the Pentagon.

Maria Telkes, physical chemist in the research division of the College of Engineering of New York University and an authority on solar energy utilization, has been named a consultant to the Stanford Research Institute.

Folke Skoog, professor of botany at the University of Wisconsin, has been selected as this year's recipient of the Stephen Hales award. Named for an early English plant physiologist, the award is granted by the American Society of Plant Physiologists to scientists who have made outstanding contributions to knowledge in the field of plant physiology. Skoog was selected to receive the award for his work on the physiology of plant hormones or auxins and for his contributions to the technique of growing tissue cultures.

The following appointments to assistant professor have been announced. West Virginia University: **Edward Dale Curry, Jr.,** and **Stanley Hunter Reed,** air science and tactics; **Harry Louis Hansen,** plant pathology and entomology; **Eugene Robert Palowitch,** mining engineering. University of Connecticut: **Philip Rosen,** physics. William Jewell College: **Glen T. Clayton,** physics. Louisiana State University: **Richard M. Paddison,** neurology.

Richard L. Dolecek, formerly associate superintendent, has been named superintendent of the solid state division at the Naval Research Laboratory, Washington, D.C. He succeeds **Wayne C. Hall,** now associate director of research for nucleonics at the laboratory.

Rafael Rodriguez-Molina, chief of medical service at the San Patricio Hospital in Puerto Rico, has been elected a fellow of the Royal Society of Tropical Medicine and Hygiene. He also has been recently chosen president-elect of the Asociacion Medica-Distrito Este, medical association of Puerto Rico.

The newly elected **fellows of the Illuminating Engineering Society** are C. E. Egeler, R. L. Oetting, and C. H. Rex of the General Electric Co.; Myrtle Fahsbender and J. J. Neidhart of the Westinghouse Electric Corp.; D. M. Finch of the University of California; Kurt Franck of the Holophane Co.; K. S. Gibson of the National Bureau of Standards; R. A. Palmer of the Duke Power Co.; and F. D. Wyatt of the Chicago Park District.

John Baird, who has been manager of analytical engineering activities at the General Electric Co.'s advanced electronics center at Cornell University, has been named manager of the newly created project analysis section at the G.E. Research Laboratory in Schenectady. The aims of the new section are to help establish the priority and scope of research effort in different scientific areas and to evaluate the results of research in order to speed their application and development by other components of the company.

Norman R. Davidson, associate professor of chemistry at California Institute of Technology, has been chosen 1954 recipient of the California section award of the American Chemical Society for his investigatons of the rates of very fast chemical reactions. He has done pioneering work in developing techniques for the study of reactions that take place in less than a thousandth of a second.

Ira G. Ross, head of the wind tunnel department at the Cornell Aeronautical Laboratory, Inc., Buffalo, has been named director of the laboratory, and **Will M. Duke,** assistant director (technical), has been appointed associate director.

Tiffany Lawyer has resigned as director of the Muscular Dystrophy Clinic at Georgetown University Hospital, Washington, D.C., to accept a clinical professorship in neurology at Columbia University and the directorship of the Montefiore Hospital Neurological Service in New York City. He has been succeeded by **Desmond S. O'Doherty,** who has been at Georgetown for 4 yr. The latter is an assistant professor of neurology and has headed the Georgetown University Neurological Service at the D.C. General Hospital, a position he is relinquishing to take over the directorship of the Muscular Dystrophy Clinic.

Rutgers University has announced the appointment of **John R. Wittenborn,** formerly an associate professor in the department of psychology at Yale University, as professor to head a new department of student personnel services within the School of Education. Wittenborn will direct a program that is designed to introduce an increased use of psychology in education both in classroom teaching and in courses in guidance and counseling. A program leading to the doctorate for prospective school psychologists will be under his direction.

Foster E. Mohrhardt, chief of the library division of the Veterans Administration's department of medicine and surgery, has been named director of the U.S. Department of Agriculture library.

N. S. Hall, professor of soils and agronomy at North Carolina State College, is on leave to serve with the biology branch of the Atomic Energy Commission. He is responsible for the phase of the AEC program concerned with soil-plant relationships and uptake of fission materials.

Frank M. Woolsey, Jr., chief of medical services at the Albany Veterans Administration Hospital and an associate professor of medicine at Albany Medical College, has been appointed associate dean and director of postgraduate medical education at the college. Woolsey will take over the duties formerly performed by **Curtland C. Brown, Jr.,** who was assistant dean of the college prior to his recent induction into the U.S. Navy, and will add others that will make him a full-time administrative assistant to the dean, Harold C. Wiggers.

Meetings

The **Technical Publishing Society,** a professional group to encourage the interchange of information among individuals engaged in preparing, editing, or publishing technical and scientific documents, has been organized in Los Angeles. Officers are A. E. Tyler, president, of the U.S. Naval Ordnance Test Station, Pasadena; Steve Stremm, vice president, of Hycon Manufacturing Co., Pasadena; John Zane, vice president, of the U.S. Navy Electronics Laboratory, San Diego. W. E. Welch, of Brubaker Manufacturing Co., Inc., Los Angeles, is secretary-treasurer of the new organization. Membership includes editors, writers, illustrators, and management and production personnel from industrial and governmental research laboratories. A board of directors is at present studying the standards to be used to accredit individual and chapter memberships.

The 37th annual meeting of the **American Council on Education,** on "Preparing to meet the rising tide of students," will take place 14–15 Oct. at the Conrad Hilton Hotel in Chicago. Among the principal speakers, Joel H. Hildebrand, professor emeritus of chemistry at the University of California, will represent the natural sciences.

The 1st national meeting of the **Institute of Management Sciences** will be held in Pittsburgh, 21–22 Oct., with headquarters at the Webster Hall Hotel. Members and other interested persons are invited to participate. The fee, including registration, a smoker, two luncheons and one dinner, is $15 for members and $20 for non-members. Further information may be obtained from Mr. Robert Johnson, c/o Touche, Noven, Bailey and Smart, 3500 Grant Bldg., Pittsburgh 30.

The institute is a new national professional society that was established in December 1953 by a nationwide group of management analysts, social scientists, mathematicians, and engineers with a common interest in the scientific analysis of management problems. The primary objectives of the institute are the identification, extension, and unification of all scientific knowledge that contributes to the understanding and practice of management. The institute is publishing a journal, *Management Science,* that will include survey

and research papers dealing with scientific analysis and theory of management. W. W. Cooper, a professor at Carnegie Institute of Technology, has been elected the first president of the organization.

The 3rd medical conference of the **Muscular Dystrophy Associations of America, Inc.,** will be held 8–9 Oct. at the New Yorker Hotel, New York.

Eight authorities from the United States and abroad will participate in the fall meeting of the **Commercial Chemical Development Association,** which will be devoted to foreign chemical development and its importance to the United States chemical industry. Emphasis will be on the interchange of technical information as a basis for commercializing chemicals in international markets. The luncheon speaker will discuss the utilitization of atomic energy for peacetime purposes, and a highlight of the meeting will be an address on the "Expanding chemicalization of foreign countries," by M. F. Mitchell of London, who is general manager of chemical industry administration for Shell Petroleum, Ltd. The meeting will be held on 7–8 Oct. at the Bedford Springs Hotel in Bedford, Pa.

Lewis L. Strauss, chairman of the U.S. Atomic Energy Commission, was the principal speaker at a founders day dinner of the **National Association of Science Writers** in New York on 16 Sept. The dinner, which was in celebration of the association's 20th anniversary, also featured reports by a group of Nobel prize winners in chemistry, physics, and medicine who, in addition to 8 surviving charter members of the association, were guests of honor. The association was founded in 1934 by 12 science writers: Howard W. Blakeslee, F. B. Colton, Watson Davis, David Dietz, Victor Henderson, Thomas Henry, Waldemar Kaempffert, Gobind Behari Lal, William L. Laurence, John J. O'Neill, Robert Potter, and Allen Schoenfeld. Blakeslee, Colton, Henderson and O'Neill have since died. At the time of the founding, these men were the only ones who were recognized as specializing in the field of science writing; today, the association has more than 100 active members who represent the nation's press associations, newspapers, and large magazines.

Roosevelt College in Chicago will hold its 5th annual **Ways of Science Conference** on 15 Oct. at which noted experts will examine the various problems encountered in interpreting science to nonscientists. Recognizing that science today is the concern of every man, conference leaders will seek methods by which science can best communicate with laymen. The conference is jointly sponsored by the college and the Philosophy of Science Association.

The 8th annual congress and festival of scientific films of the **International Scientific Film Association** will take place 6–12 Nov. at the University City and National Research Council in Rome, Italy. Outstanding films of general appeal will be shown at public performances, and there will be presentations of specialized films and papers at the meetings of the sections concerned with the films in medicine and veterinary science, in industry and technology, in scientific research, and in other subjects. Inquiries should be sent to the congress secretariat: Segreteria VIII Congresso Internazionale della Cinematografia Scientifica, Commissione per la Cinematografia Scientifica del Consiglio Nazionale delle Ricerche, Piazzale delle Scienze, 7, Rome.

Education

A 3-day program, 29 Sept.–1 Oct., bringing together some of the world's leading specialists in eye diseases, marked the opening of the new laboratories of the **Francis I. Proctor Foundation for Research in Ophthalmology** at the University of California Medical Center, San Francisco. The funds supporting the research of the foundation derive from the estate of the late Francis I. Proctor. Laboratory space for the foundation was constructed in the new Medical Sciences Building on the San Francisco campus as a result of the donation of $65,000 by Mr. and Mrs. Berthold Guggenhime of San Francisco.

A rapidly expanding collection of books in the **history of science and technology** is being established at the University of Oklahoma through the efforts of E. DeGolyer, an alumnus who is widely known for pioneer work in the application of physics to the oil industry. Stemming from an initial gift of 600 rare volumes in December 1949, the collection has grown to approximately 4000 volumes at the present time, with several hundred more being added each month. It includes such rare first editions or first printings as the major works of Vesalius, Copernicus, Aristotle, Galileo, Newton, Lavoisier, Linnaeus, Hooke, and such comprehensive works as Diderot's *Encyclopédie* and the complete *Acta Eruditorum, Histoire et Mémoires* of the French Academy, and *Philosophical Transactions*. The collection is intended as a library for research and for teaching at both the graduate and undergraduate levels. Toward this purpose the University of Oklahoma is offering courses in the history of science this present academic year.

The board of directors of the New York State Optometric Association voted to recommend a campaign to raise $400,000 toward establishment of a **school of optometry** to replace the one being closed by Columbia University in 1956. The board deplored the decision of Columbia last March to close its School of Optometry after an existence of 46 yr.

R. N. Bracewell will give a course in **radioastronomy** at the Berkeley Astronomical Department of the University of California during the autumn and spring semesters. He is a member of the Commonwealth Scientific and Industrial Organization of Sydney, Australia, and is coauthor (with J. L. Pawsey) of a new textbook on radioastronomy.

Available Fellowships and Awards

About 800 American students with special abilities in science will be selected for a year of graduate scientific study during 1955–56 in the **National Science Foundation's** 4th annual graduate fellowship program. *The closing dates for receipt of applications are 20 Dec.* for postdoctoral applicants and *3 Jan. 1955* for graduate students working toward advanced degrees.

Selections will be made solely on the basis of ability for studies in the life and physical sciences, including interdisciplinary fields. The majority of the fellowships will go to graduate students seeking master's or doctor's degrees, although about 100 awards will be made to postdoctoral applicants. Science students who are now college seniors are encouraged to apply for the awards.

Stipends will range from $1400 to $3400 depending on academic status, and dependency allowances will be made to all married fellows. Tuition and laboratory fees and limited travel allowances will also be provided.

Last year 736 selections were made out of 3300 applicants. Also, about 1400 individuals were named on an honorable mention list that was made available to the press and to deans of graduate schools. Applications may be obtained from the Fellowship Office, National Research Council, Washington 25, D.C.

Grants, Fellowships, and Awards

The **Upjohn Co.** has recently made the following research grants totaling $11,000.

Tufts College Medical School. W. Fishman, New England Center Hospital. Cancer studies, $5000.

University of Wisconsin. R. K. Meyer, dept. of zoology. Steroids and related compounds, $3000.

Johns Hopkins University. B. F. Chow, School of Hygiene and Public Health. Nutrition and metabolism, $3000.

The following **Commonwealth Fund** fellows will be in the United States in 1954–55.

T. W. Bloxam, Glasgow. Geology (metamorphic petrology), University of California.

W. Bräutigam, Heidelberg. Psychiatry, Yale University.

G. M. Brown, Oxford. Geology (igneous petrology), Princeton University.

T. N. Clifford, Leeds. Structural and stratigraphic geology, Harvard University.

P. R. Day, Bayfordburry, Hertfordshire. Plant pathology and genetics, University of Wisconsin.

L. T. Evans, Oxford. Plant growth and development, California Institute of Technology.

I. M. James, Oxford. Algebraic topology, Princeton University.

L. Mestel, Leeds. Theoretical astrophysics, Princeton University.

H. P. F. Swinnerton-Dyer, Cambridge. Algebraic number theory, Chicago.

A. J. Weir, Glasgow. Gestalt psychology, Clark University.

C. H. Wood, London. Industrial medicine, Harvard University.

E. C. Zeeman, Cambridge. Algebraic topology, University of Chicago.

K. A. R. Fastborg, Stockholm. Psychiatry, Yale University.

D. W. Davies, Teddington, Eng. Design and application of electronic digital computers, Massachusetts Institute of Technology.

L. E. J. Roberts, Harwell, Eng. Chemistry of inorganic solids, University of California.

A. D. Butcher, Melbourne. Administration of wildlife and fisheries services, University of Michigan and U.S. Department of Interior's Wildlife Service.

R. G. Downes, Kew, Australia. Ecology and administration of conservation, University of Michigan and U.S. Soil Conservation Service.

A. T. Johns, Palmerston, North, New Zealand. Microbiology, University of California.

A. I. McCutchan, Brisbane, Australia. Dam construction and water control, U.S. Bureau of Reclamation, Denver.

A. L. C. Thorne, Vom, Northern Nigeria. Research in vaccine production and public health measures for the control of rabies, U.S. Public Health Service's Communicable Diseases Center, Montgomery, Alabama.

H. L. Kornberg, Sheffield. Biochemistry research, City of N.Y. Public Health Laboratories, Inc., 9 mo.

E. A. Power, London. Mathematical methods used in theoretical physics, Princeton University, 4 mo.

G. C. Shephard, Birmingham, Eng. Algebraic geometry and abstract algebras, University of Chicago, 9 mo.

R. L. Williams, Oxford. Infrared spectroscopy, University of California, 4 mo.

The **National Research Corporation Scientific Trust,** Cambridge, Mass., has made a grant of $3980 to the University of California at Riverside for the support of basic research. The work is to be conducted by James N. Pitts, recently appointed to the post of associate professor of chemistry, and relates to photochemical studies in the far ultraviolet and Schumann regions. This is a continuation of the work conducted by Pitts at Northwestern University under another grant from the trust.

The **Atomic Energy Commission** has announced the award of 20 unclassified physical research contracts; three are new, and the remainder are renewals. The contracts are generally for a term of 1 yr.

University of Delaware. R. L. Pigford. Thermal diffusion in liquids, $6155.

Rutgers University. S. Weissmann. Radiation damage of metals and alloys, $10,983.

University of Washington. G. H. Cady. Preparation of compounds containing O-F or N-F Bonds, $7100.

Carnegie Institute of Technology. R. Smoluchowski. Grain boundaries and lattice imperfections, $36,277. Radiation effects on materials, $40,130.

Carnegie Institute of Technology. T. P. Kohman. Nuclear chemistry research, $42,552.

Columbia University. J. L. Kulp. U-Pb method of age determination, $35,723.

Cornell University. J. L. Hoard. Structure of fluorocarbons, elementary boron, and boron compounds, $5171.

Florida State University. R. E. Johnson. Exchange between labeled halogens and certain inorganic halides, $4543.

Oregon State College. T. H. Norris. A study of generalized acid-base phenomena in nonaqueous ionizing solvents with radioactive tracers, $7750.

University of Oregon. P. Van Rysselberghe. Corrosion of zirconium, $11,200.

Pennsylvania State University. T. F. Bates. An investigation of the mineralogy and petrography of uraniferous shales and lignites, $86,014.

Pennsylvania State University. H. D. Wright. Mineralogy of uranium-bearing deposits in the Boulder Batholith, Montana, $8300.

Princeton University. M. G. White. 18-mev cyclotron and associated nuclear physics research, $221,190.

University of Rochester. R. E. Marshak. Synchrocyclotron, cyclotron, and theoretical physics research, $669,040.

Syracuse University. P. Barrett. Nuclear interactions of cosmic rays, $23,922.

Tufts College. T. R. P. Gibb, Jr. Research on hydrides, $26,720.

State College of Washington. H. W. Dodgen. The formulae and stability of complex ions in solution, $5154.

Yale Unviersity. H. L. Kraybill and E. C. Fowler. High-energy physics, $34,506.

Yale University. F. Hutchinson. Stopping power of water, $3000.

Miscellaneous

A permanent and unique record of the largest vaccination campaign ever undertaken is now available to tuberculosis specialists and public health workers as a result of the publication by the World Health Organization of reports covering mass **BCG vaccination** against tuberculosis in 23 countries. A total of nearly 30 million persons were tested with tuberculin, and almost 14 million of them received BCG vaccination against tuberculosis in the course of programs conducted for 3 yr by the International Tuberculosis Campaign.

Data on each person tested and vaccinated were recorded on individual cards which then were tabulated according to sex and age. These statistics were collected and analyzed by the WHO Tuberculosis Research Office in Copenhagen. Publication of all the reports concerning the mass campaigns conducted from 1948 to 1951 is now complete and represents an important step in the systematic and carefully controlled investigation of BCG vaccine and vaccination undertaken by WHO.

Since June 1951, the work of the International Tuberculosis Campaign has been officially the responsibility of the World Health Organization and the UN Children's Fund. The campaign has now spread to 30 other countries, where tuberculin tests have been given to 90 million people, of whom 36 million have received BCG vaccination. The WHO Research Office is also analyzing the results from these vaccination campaigns.

With the September issue of the National Mental Health Program's **Progress Report,** the National Institute of Mental Health concludes its sponsorship of the monthly newsletter. Henceforth, the features of the *Progress Report* will appear as a two-page section of the monthly *NAMH Reporter,* which is distributed by the National Association for Mental Health, Inc., 1790 Broadway, New York 19, a nonprofit organization. Those who wish to receive the *Reporter* should address their requests to the association.

The **Marine Biological Laboratory,** Woods Hole, Mass., will be open for advanced biological research and to library readers throughout the year. Furnished housekeeping apartments and rooms will be available to those desiring such facilities. Inquiries should be addressed to the laboratory.

A plea for "letters, paintings, and other materials reposing in shops, private libraries, and homes" that are the work of the 18th century ornithologist **John Abbot** has been made by Elsa Allen, research associate in ornithology at Cornell University who is conducting a study of Abbot's life and work. She has been working for the past 2 yr on this project and now has assembled over 1000 colored slides of Abbot's unpublished bird paintings.

John Abbot came to America in 1773 from his London home and lived for over 60 yr in and near Savannah. After the Revolution he entered upon a long career of painting and studying American birds, insects, and plants and is said to have accomplished more work than any other naturalist in America at that time. Because he did not publish his paintings and texts, these have lain for 150 yr in several different libraries. Both Harvard University and the University of Georgia at Athens possess fine sets of his bird drawings, but the main parts of his vast collections are in England.

The **Dodrill-GMR mechanical heart,** introduced in 1952 and the first device of its kind to be used successfully on human patients undergoing heart surgery, has been presented as a permanent exhibit to the Smithsonian Institution. The device was conceived and developed by a team headed by F. D. Dodrill of Detroit's Harper Hospital and consisting of other Harper medical men and engineers from the General Motors Research Laboratories.

Necrology

Samuel W. Boggs, 65, author and geographer for the State Department, Washington, D.C., 14 Sept.; **Simeon L. Carson,** 72, former clinical professor of surgery at Howard University Medical School, Washington, D.C., 8 Sept.; **W. Roy Geddes,** 57, vice president of North American Cyanamid, Ltd., Toronto, Canada, 12 Sept.; **John E. Hoyt,** 75, author, research engineer, and retired professor of physics at Drexel Institute of Technology, Philadelphia, Pa., 12 Sept.; **Edwin C. Kenton,** vice president of Evans Research and Development Corp., New York, 19 Aug.; **John H. Kintner,** 62, investigator of animal diseases and director of the Grayson Foundation Laboratory at College Park, Md., 28 Aug.

Charles A. McKendree, 67, psychiatrist and retired professor of clinical neurology at the College of Physicians and Surgeons, Columbia University, New York, 11 Sept.; **Charles McMartin,** 74, head of the department of surgery and former director of the department of dermatology and urology at Creighton University, Omaha, Nebr., 14 Sept.; **Lillian B. Patterson,** dean of the University of Washington School of Nursing, Seattle, Wash., 8 Sept.; **Harold R. Ranken,** 71, retired chief engineer of the Leeds and Northrup Co., Philadelphia, Pa., 14 Sept.; **William G. Tuller,** 35, radar and electronics investigator and vice president in charge of engineering for Melpar, Inc., Alexandria, Va., 5 Sept.; **Otis R. Wolfe,** 68, chief eye surgeon of the Wolfe Eye Clinic, Marshalltown, Iowa, 11 Sept.

Erratum. The statement attributed to Joshua Lederberg in the report of the Oak Ridge Conference on Genetic Recombination [*Science* **120**, 291 (20 Aug.)] to the effect that streptomycin treatment will improve the crossing of certain strains of bacteria, was in error. There is no demonstrated action of streptomycin on the fertility of *E. coli* crosses.—B. G.

Book Reviews

The Freshwater Fishes. vol. I. Key to the game and commercial fishes of the Province of Quebec. Vianney Legendre. Quebec Biological Bureau, Montreal, Engl. ed. 1, 1954. 180 pp. *Les Poissons d'Eau Douce,* Tome 1. Deuxième édition française.

This is an excellent book dealing with the identification of the fresh-water fishes of the Province of Quebec. The key characters are splendidly illustrated. There are 74 full-page illustrations of species, mostly reproduced from the State of New York Biological Survey. The distribution of some of the species in the Province is shown on page-size maps. There is a detailed glossary, a partial bibliography, and a complete index. The printing is well done on good paper.

Some ichthyologists may take exception to the systematics, which are apparently based largely on the work of L. S. Berg. Although I am in sympathy with "condensation," I am inclined to be reluctant to make changes until substantial data are presented in support of such. For example, I accept "Salvelinus namaycush" in view of the paper by Morton and Miller in *Copeia* [No. 2, 5 May 1954]. On the other hand, I hesitate in regard to the suppression of the family Coregonidae and the genera *Leucichthys* and *Prosopium*. However, these are matters that are open for discussion among specialists and do not detract from the great usefulness of the publication.

The author is to be congratulated on the production of a contribution of outstanding merit.

W. A. Clemens
Institute of Fisheries, University of British Columbia

Advances in Protein Chemistry. vol. VIII. M. L. Anson, Kenneth Bailey, and John T. Edsall, Eds. Academic Press, New York, 1953. 529 pp. Illus. $10.50.

The arrival of a new volume of *Advances in Protein Chemistry* has become an important event in the biochemical year. The latest of a worthy line, volume VIII maintains the quality of its predecessors. The number of contributors is not large, but the scope ranges from virus reproduction to physical methods. Perhaps the most dominant theme is that of protein synthesis. In three of the papers, those by Borsook, Putnam, and Gale, this vital problem is approached from quite different viewpoints.

Bricas and Fromageot give an exhaustive account of naturally occurring peptides of bacterial, plant, and animal origin. Glutathione is treated in some detail as well as the pteroylglutamic acid family. A useful table of uncommon amino acids is included.

Borsook's study on peptide-bond formation examines the energetics of the process in different classes of peptide synthesis. The question of amino-acid incorporation into proteins by exchange or by synthesis of new protein is discussed.

Putnam contributes a fine article on the nature and reproduction of bacteriophages. As a point of convergence of so many different disciplines, the phages are a fruitful, if difficult, field for survey. Biochemical studies of the infected cell are correlated with the results of electron microscopy and cytological studies, with an emphasis on the isotopic tracer technique.

Gale summarizes the work of his school on the assimilation of amino acids by bacteria and its relation to protein and nucleic-acid synthesis. On the basis of the effect of drugs and antibiotics on these processes, Gale has been able to put forward a tentative integrated scheme.

In an article unusual for the series, Arthur discusses the properties and economic importance of the peanut proteins. Since the annual crop contains about 3 million tons of these proteins, and new applications of this abundant raw material are increasing, particularly in the artificial fiber field, the article is well warranted.

Weber gives the most comprehensive account of his new fluorescence polarization method yet published. The relative experimental simplicity of the method and its independent approach make it a valuable addition to the methods already available in the field of protein molecular shapes and sizes and should insure a far wider application than it has so far enjoyed.

In their paper on zone electrophoresis, Tiselius and Flodin summarize recent developments in the separation of ions on paper by an electric field, a technique that is assuming an importance comparable with paper chromatography. The application to proteins, which is discussed, shows signs of becoming a precise method.

R. H. Locker
Department of Scientific & Industrial Research, Wellington, New Zealand

A Monograph of the Fungus Genus Cercospora. Charles Chupp. Charles Chupp, Ithaca, N.Y., 1954. 667 pp. Illus. $10.

This long-awaited monograph of the genus *Cercospora* is the culmination of more than 30 years of painstaking study by Charles Chupp in field, laboratory, and herbarium in the United States and Europe. Chupp, a distinguished plant pathologist and former president of the American Phytopathological Society, is the recognized authority on *Cercospora*.

The book is prefaced by an exceptionally clear discussion of the characters that define the genus and its species and by definitions of the taxonomic criterions employed in the species' descriptions. More than 1900 species of *Cercospora* are listed according to the principal host attacked; the hosts are arranged alphabetically by family. Each species is fully described, its hosts listed, the type collection identified, the geo-

graphic distribution given, and significant features noted. Wherever several species are reported on a single host genus, a key is given. Many new species, with Latin descriptions, are recorded, as are a number of new combinations. Separate and complete indexes by host and by *Cercospora* species conclude the book. This important treatise, which has been prepared with exceptional care, should be a part of the working library of all plant pathologists and mycologists.

A. W. DIMOCK
New York State College of Agriculture

The Collected Papers of Peter J. W. Debye. Interscience, New York–London, 1954. xxi + 700 pp. Illus. $9.50.

Debye's classical papers are scattered among many journals covering a period of almost 50 years. The collection in a single volume of 51 of his papers, selected by Debye himself, is an important and welcome addition to the literature of science.

It is almost impossible for present-day natural scientists to be unaware of Debye's work; his theories are included in standard books on many phases of physics and chemistry. An acquaintance with these theories is greatly enriched by reading the original papers. Debye's treatment of a problem, from its historical background to its final solution, displays the refreshing clarity familiar to those who have heard his lectures. Through reading the original papers, one gains an appreciation of Debye's unique ingenuity and resourcefulness in attacking problems where methods more conventional than his had failed.

The book is divided into several sections. The division on "X-ray scattering" contains 11 papers, including the Debye-Scherrer treatment of scattering from crystal powders. The seven papers under "Dipole moments" contain the theories that relate molecular structure and intermolecular forces to the basic electric properties of the molecules. Under the heading "Electrolytes" are 11 papers that develop the famous Debye-Hueckel theory and some of its consequences.

The section on "Light scattering" consists of papers on the development and application of the technique that, in the last 10 years, has probably become the most useful method for obtaining absolute values of molecular weights and dimensions of macromolecules. Somewhat unexpectedly, papers on hydrodynamic properties of polymers and on reaction rates in ionic solutions are also included in this section.

Among the 10 articles in the "Miscellaneous" group are the well-known papers on the specific heats of crystals and on the possibility of reaching very low temperatures by adiabatic demagnetization. These sections also contain two papers that, as technical reports to the Rubber Reserve Company, were hitherto relatively inaccessible: one on the angular dissymmetry of light scattering, the other on the determination of molecular weights by the application of inhomogeneous electric fields.

An introductory section includes a biographic sketch by R. M. Fuoss, followed by brief introductions to the various sections by H. Mark, C. P. Smyth, and R. M. Fuoss.

Papers originally published in Dutch or German have been translated into English and were reproduced from typescript. Those papers originally in English were reproduced directly from the journals in which they appeared. Typographically, this volume is adequate; the translation is almost flawless. In the very few places where I found the translation awkward, the type uneven, or oversights in proofreading, the meaning of the text was never affected and the clarity never obscured.

The Collected Papers of Peter J. W. Debye is highly recommended to the serious student, the teacher, and the research worker interested in gaining an insight into the workings of nature on the molecular level. It is probably no exaggeration to state that these papers have stimulated, directly or indirectly, the greater part of modern research on molecular structure and interactions. Contained in this convenient collection, Debye's papers will be readily available to inspire additional exploration.

ULRICH P. STRAUSS
School of Chemistry, Rutgers University

Dynamics of Growth Processes. L. M. Kozloff *et al.* Edgar J. Boell, Ed. Princeton Univ. Press, Princeton, N.J., 1954. vii + 304 pp. Illus. + plates. $7.50.

The Society for the Study of Development and Growth organizes annual symposiums in its field of interest. The 11th Growth Symposium was held in June 1952 at Williams College, in cooperation with the Committee on Developmental Biology of the National Research Council. This volume presents the papers given at that meeting on the subject of dynamics of growth processes. It will be recalled that the earlier symposiums of this series appeared as supplements in the journal *Growth*, and the continuity of the present volume with these earlier publications is especially emphasized by E. J. Boell in his foreword.

The extraordinarily wide range in the scope of the 13 papers that constitute this volume makes the reviewer's task rather difficult. The symposium deals with growth phenomena in viruses (L. M. Kozloff), bacteria (A. Novick and L. Szilard), the cytochemistry of protein synthesis (A. W. Pollister), interactions between nucleus and cytoplasm during growth (G. Fankhauser), differentiation in relation to growth among animals (K. R. Porter) and plants (D. S. Van Fleet), the physical (F. W. Went) and chemical (F. Skoog) regulation of growth in plants, the chemical control of growth in animals (R. Gaunt), the relationship between skeletal growth and development in children (W. W. Greulich), growth rhythms and allometry (D. A. Sholl), hereditary mechanisms in animal growth (G. E. Dickerson), and the mathematical aspects of population growth (F. E. Smith).

Since all the contributors are eminent authorities in their respective fields, the volume undoubtedly rep-

resents a most informative and up-to-date summary of the dynamics of growth processes. Each of the articles is followed by a carefully selected list of references, and numerous diagrams and photographs illustrate the text. In my opinion it is regrettable, however, that the formal presentations are not followed by discussions, since the critical analysis of each topic by the other members of this highly competent panel would have been particularly informative. In any event, there can be no doubt that the proceedings of this symposium, like those of its predecessors in *Growth*, can be highly recommended to all those interested in growth phenomena.

HANS SELYE
Institute of Experimental Medicine and Surgery, Université de Montréal

Progress in the Chemistry of Fats and Other Lipids. vol. 2. R. T. Holman, W. O. Lundberg, and T. Malkin, Eds. Academic Press, New York; Pergamon Press, London, 1954. 347 pp. Illus. + plates. $9.80.

The articles contained in this volume, like those in volume I, were originally intended for a new edition in English of the Hefter-Schoenfeld *Chemie und Technologie der Fette und Fettprodukte*. Each chapter is an authoritative survey of a specific area of research on lipids.

Physical aspects are discussed in "The polymorphism of glycerides," by T. Malkin, and "The surface properties of fatty acids and allied substances," by D. G. Dervichian. The first, a continuation of a chapter in volume I, discusses present views on glyceride polymorphism based on thermal and x-ray studies. The second surveys the studies on surface properties of fatty acids, salts, and simple esters of fatty acids, glycerides, and phosphatides. "Autoxidation of fats and related substances," by R. T. Holman, is concerned with the chemical aspects of this phenomenon. Possible mechanisms of autoxidation are discussed in detail.

Biochemical methods are represented by the chapters "Infrared absorption spectroscopy in fats and oils," by D. H. Wheeler, and "Countercurrent fractionation of lipids," by H. J. Dutton. The former is concerned with the utilization of infrared absorption in studying the structures of lipids. The latter is a discussion of the fundamentals of countercurrent distribution, a description of the apparatus used in this method, and a survey of the application of this technique to the fractionation of lipids.

In "Urea inclusion compounds of fatty acids," by H. Schlenk, the physical and chemical properties of these compounds are described. The use of these adducts in fractionating lipids and determining lipid structures is also discussed.

A very comprehensive survey is made by H. J. Deuel in "Nutritional significance of the fats." This deals primarily with the role of fats in physiological functions and with the factors affecting the nutritional value of fats.

This volume is generally well written and thoroughly documented. It would be an excellent addition to the library of anyone actively engaged or generally interested in lipid research.

HENRY H. TOMIZAWA
Department of Medicine, University of Washington

Organic Chemistry. Reynold C. Fuson and H. R. Snyder. Wiley, New York, and Chapman & Hall, London, 1954, ed. 2. viii + 544 pp. Illus. $6.50.

This new edition of a well-known college textbook in introductory organic chemistry is very similar to the first edition in size, in the arrangement and titles of its chapters, and in the presentation of a unified treatment of aliphatic and aromatic compounds. The plan of presenting in the early chapters a bird's-eye view of the general field of organic chemistry, followed by a more mature and well-rounded treatment in the subsequent chapters, is retained; but the new edition is not formally divided into part I and part II as was the first edition.

By eliminating sections on methods of preparation and by using cross references to reactions previously studied, rather than duplicating the reactions, space has been provided in the new edition for new material. In making these changes, the authors have given special emphasis to the inclusion of theoretical material and to the more extensive treatment of several topics, such as the reactions of halogen compounds and polymerization.

As in the first edition, much of the second half of this book is devoted to the reactions of certain functional groups. The reactions are grouped and treated according to type rather than discussed on the basis of common starting materials, end-products, or utilization in industry and allied fields of science. Whereas this feature of the earlier edition has been found attractive by teachers of organic chemistry, particularly those whose classes are made up mainly of students preparing for a professional career in organic chemistry, I feel that many of the students who take a full-year course in organic chemistry in preparation for medicine or engineering will find several of the chapters in this portion of the book too detailed and exhaustive in reaction coverage. Not enough attention is given to the development of the ideas and concepts that have become the basis for reasoning in structural and functional organic chemistry. Modern mechanistic concepts are used in explaining many of the reactions, but little or no basis for accepting such concepts is developed. For example, no clear exposition of the concept of resonance is offered; and the role of energy, varying bond strengths, and dipole moments in organic reactions is completely ignored. In my experience, most students, including prechemists, find that the development and use of such ideas and concepts in introductory organic chemistry are much more stimulating than the task of learning or cataloging all the many reactions of a particular functional group or type of compound.

The first half of the book will appeal to many because of its simplicity of presentation. The language, homologous series, and simpler reactions of the organic chemist are logically and lucidly introduced in rapid manner. However, students who must leave organic chemistry with only such a survey of the field will take with them an impression of "frozen simplicity" that is very unrealistic.

Paper, presswork, and binding in the edition are first class.

CHARLES R. DAWSON
Department of Chemistry, Columbia University

Cohort Fertility. Native white women in the United States. Pascal K. Whelpton. Princeton Univ. Press, Princeton, N.J., 1954. xxv + 492 pp. Illus. $6.

This volume of statistics deals with the cumulative fertility of groups of women as they live through the childbearing period. It covers the period from 1920 and is based upon official census and vital statistics data. These were examined especially with respect to order of birth in relation to maternal age. On the basis of the data, fertility tables were constructed for hypothetical groups of women for each year since 1920. The official figures were adjusted to allow for nonregistered births and for other factors, such as understatements regarding birth order and the number of children ever born.

The author is an authority in his field. The volume is well documented and contains an excellent table of contents that obviates the need for an index. Summaries appear at the ends of the chapters. The graphs are well drawn and the book is printed in large offset type. It is recommended to students interested in population trends.

DOUGLAS P. MURPHY
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Methods in Medical Research. vol. 6. J. Murray Steele, Ed. Year Book Publ., Chicago, 1954. xiii + 271 pp. Illus. $7.

Volume VI, like its predecessors, is a useful and authoritative collection of articles dealing with various aspects of methodology applicable to the vast field of medical research. It contains four sections representing the collaborative efforts of 12 authors.

Section I, "Some methods of studying human genetics," by C. C. Li, deals with analytic techniques, tracing pedigrees chiefly through observation of clinical cases. Although at times the treatment becomes too mathematical to be appreciated by the average reader, especially the article on "Segregation of recessive offspring," nevertheless the author has given a clear presentation of recent developments.

The second section is a discussion of "Methods in environmental medical research," which covers a number of aspects, written by several authors (R. L. Pratt and A. Henschel; L. D. Carlson; A. T. Miller, Jr.; J. D. Hardy and Alice M. Stoll; and S. Robinson and Aline H. Robinson). They examine the applicability of the physical methods to physiological research that are most common to all studies of the effects of climatic conditions.

Section III is an extensive article on "Statistics in medical research," by D. Mainland and L. Herrera. It includes various phases of statistical analysis. In his introduction to this chapter, D. Mainland expresses the opinion that "what most medical research workers need is not, primarily, more knowledge of statistical tests but a realization of what modern biological statistics implies throughout the conduct of any type of medical investigation." It can be said that the authors have succeeded in fulfilling their objective in presenting the subject from a broad and dynamic point of view. This chapter should prove very useful for anyone doing medical research requiring statistical interpretation.

Section IV is entitled "Designs and construction of metabolic cages," by A. Lazarow. Cage design for the common laboratory animals—namely rat, mouse, dog, and monkey—is discussed. Here Lazarow has gathered together the literature concerning design of equipment for metabolic study of these animals, especially the rat. The discussion is of a practical nature and can be consulted profitably by anyone trying to build such equipment.

A surprisingly large amount of information has been compressed within the pages of a rather small book. The material has been skillfully and clearly presented and should prove very helpful to research workers in the field of medicine.

W. Z. HASSID
Department of Plant Biochemistry, University of California, Berkeley

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Books Reviewed in THE SCIENTIFIC MONTHLY

October

Principles of Biology, W. G. Whaley, O. P. Breland, C. Heimsch, A. Phelps, and G. S. Rabideau (Harper). Reviewed by Robert B. Gordon.

Plague, R. Pollitzer (World Health Organization; Columbia Univ. Press). Reviewed by K. F. Meyer.

The Magic of Electronics, E. J. Bukstein (Ungar). Reviewed by R. C. Retherford.

Physics Principles, S. S. Ballard, E. P. Slack, and E. Hausmann (Van Nostrand). Reviewed by R. F. Paton.

Introduction to College Physics, R. D. Rusk (Appleton-Century-Crofts). Reviewed by W. Paul Gilbert.

First Year College Physics, C. E. Bennett (Ronald). Reviewed by R. F. Paton.

An Introduction to Bacterial Physiology, E. L. Oginsky and W. W. Umbreit (Freeman). Reviewed by Philipp Gerhardt.

A French–English Dictionary for Chemists, A. M. Patterson (Wiley and Chapman & Hall, ed. 2). Reviewed by William C. Boyd.

General Chemistry, E. G. Rochow and M. K. Wilson (Wiley; Chapman & Hall). Reviewed by Frank D. Martin.

Introduction to Chemistry, R. T. Sanderson (Wiley; Chapman & Hall). Reviewed by Robert B. Fischer.

Principles of Polymer Chemistry, P. J. Flory (Cornell Univ. Press). Reviewed by H. Mark.

Representative Chordates, C. K. Weichert (McGraw-Hill). Reviewed by J. Wendell Burger.

Manual of Child Psychology, L. Charmichael, Ed. (Wiley and Chapman & Hall, ed. 2). Reviewed by H. E. Weaver.

Methods of Research: Educational, Psychological, Sociological, C. V. Good and D. E. Scates (Appleton-Century-Crofts). Reviewed by Frank W. Hubbard.

Introductory College Mathematics, A. Leonhardy (Wiley; Chapman & Hall); ***Introductory College Mathematics,*** C. G. Jaeger and H. M. Bacon (Harper). Reviewed by H. S. Pollard.

Inorganic Quantitative Analysis, C. W. Griffin (Blakiston, ed. 2). Reviewed by Jay A. Young.

Rocks and Mineral Deposits, P. Niggli, trans. by R. L. Parker (Freeman). Reviewed by Howard A. Meyerhoff.

Petrography: An Introduction to the Study of Rocks in Thin Sections, H. Williams, F. J. Turner, and C. M. Gilbert (Freeman). Reviewed by J. Robert Berg.

Physical Geology, L. D. Leet and S. Judson (Prentice-Hall). Reviewed by J. R. Macdonald.

The Juvenile in Delinquent Society, M. L. Barron (Knopf). Reviewed by James L. McCartney.

Treatise on Invertebrate Paleontology, R. C. Moore, Ed., pt. G, *Bryozoa,* R. S. Bassler (Univ. of Kansas Press). Reviewed by John Imbrie.

Manual of the Plants of Colorado, H. D. Harrington (Sage). Reviewed by Harriet G. Barclay.

College Botany, H. J. Fuller and O. Tippo (Holt, rev. ed.). Reviewed by Howard E. McMinn.

Vertebrate Dissection, W. F. Walker, Jr. (Saunders). Reviewed by Harvey I. Fisher.

New Books

Chemical Pathways of Metabolism. vol. II. David M. Greenberg, Ed. Academic Press, New York, 1954. vii + 383 pp. Illus. $9.50.

Qualitative Analysis Using Semimicro Methods. Esmarch S. Gilreath. McGraw-Hill, New York, 1954. viii + 287 pp. Illus. $4.25.

Le Magnétisme des Corps Célestes. A. Dauvillier. vol. 1, *Magnétisme Solaire et Stellaire Couronne Solaire et Lumière Zodiacale.* 171 pp. Illus. + plates. Paper, F. 1600. vol. 2, *Variations et Origine du Géomagnétisme.* 161 pp. Illus. + plates. Paper, F. 1500. Hermann, Paris, 1954.

Highway Engineering. Laurence I. Hewes and Clarkson H. Oglesby. Wiley, New York; Chapman & Hall, London, 1954. xi + 628 pp. Illus. $8.

Dictionary of Linguistics. Mario A. Pei and Frank Gaynor. Philosophical Library, New York, 1954. 238 pp. $6.

Successful Commercial Chemical Development. H. M. Corley, Ed. Wiley, New York; Chapman & Hall, London, 1954. xxv + 374 pp. $7.75.

Indians of the Plains. American Museum of Natural History Anthropological Handbook No. 1. Robert H. Lowie. McGraw-Hill, New York, 1954. xiii + 222 pp. Illus. $4.75.

Amphibians and Reptiles of Western North America. Robert C. Stebbins. McGraw-Hill, New York, 1954. xxii + 528 pp. Illus. + plates. $8.50.

God and Space-Time. Deity in the philosophy of Samuel Alexander. Alfred P. Stiernotte. Philosophical Library, New York, 1954. xxvii + 455 pp. $5.

Protozoology. Richard R. Kudo. Charles C. Thomas, Springfield, Ill., ed. 4, 1954. xi + 966 pp. Illus. $10.75.

Mathematical Puzzles and Pastimes. Aaron Bakst. Van Nostrand, New York–Toronto; Macmillan, London, 1954. vi + 206 pp. Illus. $3.75.

The Vitamins: Chemistry, Physiology, Pathology. vol. II. W. H. Sebrell, Jr., and Robert S. Harris, Eds. Academic Press, New York, 1954. xiii + 766 pp. Illus. $16.50.

Diagrams in Punched Card Computing. Fred Gruenberger. Univ. of Wisconsin Press, Madison, 1954. ix + 108 pp. Illus. Paper, $3.75.

Metabolic Interrelations. With special reference to calcium. Trans. of the Fifth Conference, 5–6 Jan. 1953. Edward C. Reifenstein, Jr., Ed. Josiah Macy, Jr. Foundation, New York, 1954. 386 pp. Illus. $5.

A Devotion to Nutrition. Frederick Hoelzel. Vantage Press, New York, 1954. 88 pp. $2.50.

Elements of Food Engineering. vol. 3, Unit Operations, pt. 2. Milton E. Parker; with the collaboration of Ellery H. Harvey and E. S. Stateler. Reinhold, New York, 1954. 241 pp. Illus. $6.75.

The Rise and Fall of Maya Civilization. J. Eric S. Thompson. Univ. of Oklahoma Press, Norman, 1954. xii + 287 pp. Illus. + plates. $5.

Fiberglas Reinforced Plastics. Ralph H. Sonneborn; with chapters by Albert G. H. Dietz and Alton S. Heyser. Reinhold, New York, 1954. xii + 244 pp. Illus. $4.50.

Miscellaneous Publications

Beiträge zur Kenntnis der Rotliegendflora Thüringens. pt. 2 Sitzungsberichte der Deutschen Akademie der Wissenschaften zu Berlin, Klasse für Mathmatik und allgemeine Naturwissenschaften, Jahrgang 1953, No. 3. Winfried Remy. 20 pp. Illus. + plates. DM. 2.50. ***Statistische Thermodynamik des Vertikalen Temperaturgradienten der Atmosphäre.*** No. 4. Hans Ertel. 14 pp. DM. 1. Akademie-Verlag, Berlin, 1954.

The All-India Teak Seed Origin Sample Plots. Indian Forest Bull. No. 177. G. S. Mathauda. Manager of Publications, Delhi, 1954. 14 p. Illus.

Growing With the Years. New York State Joint Legislative Committee on Problems of the Aging, Newburgh, N.Y., 1954. 159 pp. Illus. Gratis.

Reducing Tool and Tool Service Costs with a Tool Loan Receipt System. Bull., Engineering Expt. Sta. Ser., No. 93. C. A. Horst. Virginia Polytechnic Inst., Blacksburg, 1954. 31 pp. Illus. 25¢.

Proceedings and Papers of the Twenty-Second Annual Conference of the California Mosquito Control Association. 2–4 Dec 1953. Harold Farnsworth Gray, Ed. The Association, Turlock, Calif., 1954. 107 pp. Illus.

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Technical Papers

Some Observations on the Electrolyses of Solutions of Rare-Earth Metal Salts in Basic Solvents*

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In an earlier discussion (*1*) of the general problem of electrodepositing the rare-earth metals from nonaqueous solvents, it was pointed out that successful depositions will most probably utilize highly basic solvents. Preliminary measurements using anhydrous ethylenediamine, monoethanolamine, and morpholine indicated that although anhydrous rare-earth metal acetates are comparatively soluble in these mediums, they behave as weak electrolytes. On the other hand, the nitrates, bromides, and iodides, although less soluble, are stronger electrolytes. Since both ethylenediamine and monoethanolamine gave excellent promise as electrolytic solvents for rare-earth metal salts, electrolyses studies in these mediums have been carried out. Anhydrous morpholine was not so investigated because of its poorer electrolytic solvent properties.

Anhydrous yttrium acetate, neodymium bromide, lanthanum nitrate, and impure neodymium iodide, prepared as previously described (*1*), were electrolyzed in anhydrous ethylenediamine and monoethanolamine in a 60-ml cell of the type described by Davidson and Jirik (*2*). Electrodes used were of bright platinum and were separated by a medium porosity glass disk. A potential difference of 110 v was employed, and the cell was maintained by external cooling at room temperature or no more than 25°C above room temperature. Gases generated were allowed to escape through tubes, the ends of which were immersed in paraffin oil. In a few instances, small quantities of water were added to increase conductivities. After each electrolysis, the cathode was immersed in dry, peroxide-free ether to remove the solvent. It was then transferred to a nitrogen-filled dry box, where the deposit was removed mechanically. Rare-earth metal contents were determined by weighing as oxides; reducing powers were measured with standard acidic permanganate solutions; and x-ray diffraction patterns were determined by using samples contained in sealed cellophane tubes.

Electrolyses of ethylenediamine solutions gave cathode deposits with all salts tested, but no deposits were obtained with monoethanolamine solutions. Because of reduced solubilities and conductivities, no deposit weighing more than 30 mg was obtained. Such deposits were shown by analysis to contain the respective rare-earth metals. All deposits possessed metallic characteristics and were smooth, the lanthanum and yttrium materials being silvery gray to black in color and the neodymium materials golden yellow. Comparable colors have been reported previously for the free metals (*3*). All deposits oxidized in air or in contact with water and evolved hydrogen from hydrochloric acid solution. Each was strongly reducing toward the permanganate ion. These are all properties of the free rare-earth metals. Results of typical electrolyses are summarized in Table 1.

The small quantities of cathode deposits available for study and their marked chemical reactivities precluded highly accurate analytic evaluations. Gravimetric data indicated average rare-earth metal contents of about 50 percent. Permanganate titrations showed the presence of reducing agents of relatively small equivalent weights—materials that might have been free metals, hydrides, or unipositive compounds. Since the deposits showed none of the properties of hydrides—for example, spontaneous flammability—and unipositive derivatives of these metals have not been described, the presence of free metals is logical. The reducing abilities of the various neodymium deposits averaged to 51 percent free metal—a figure remarkably close to that obtained by direct analysis. Microanalyses of cathode deposits showed the presence of substantial quantities of carbon, hydrogen, and nitrogen but in no consistent or logical atom ratios. It appears, therefore, that the free metals were deposited together with trapped or adsorbed organic materials. This is not uncommon when metals are deposited from organic mediums (*4*). Some reactions

Table 1. Data for typical electrolyses in ethylenediamine.

Solution	Current density (ma cm^{-2})	Time (hr)	Comment on deposit
0.185*N* $Y(C_2H_3O_2)_3$ + 6 drops H_2O in 60 ml	0.20	46	White, dendritic, forming after 18 hr
Saturated $Y(C_2H_3O_2)_3$	1.6 –0.32	96	Iron-gray, smooth, metallic
Saturated $Y(C_2H_3O_2)_3$	1.6 –0.32	48	Iron-gray, metallic
Saturated $Y(C_2H_3O_2)_3$ + 1 drop H_2O in 60 ml	0.29–0.04	96	Hard, smooth, crystalline, metallic, adhering strongly to platinum cathode
Saturated $NdBr_3$	5.4 –1.95	16	Golden yellow, lustrous, adherent
Saturated $NdBr_3$	1.95–0.36	72	Golden yellow, lustrous, metallic
Saturated $NdBr_3$	2.2 –0.075	48	Golden yellow, metallic plate
Saturated $La(NO_3)_3$	4.9 –0.33	18	Smooth, black, with some white incrustation near surface of solution

with oxygen, moisture, and carbon dioxide undoubtedly occurred during handling.

Cathodic evolution of hydrogen in small quantities decreased current efficiencies but did not preclude deposition of the metals. In ethylenediamine, hydrogen reduction is generally insignificant. Simultaneous hydrogen and metal deposition have been noted in many cases—for example from chromium and iron salts in liquid ammonia (*5*) and from rare-earth salts in absolute ethanol (*6*). Addition of up to 4 drops of water per 60 ml of solution facilitated formation of black, lustrous deposits from yttrium acetate solutions, but larger quantities gave hydrous yttrium oxide. The accelerating effects of water have been noted in other systems. Thus, copper is plated from moist copper (II) nitrate solutions in liquid ammonia but not from anhydrous solutions, presumably because of greater ease of reduction of the $[Cu(H_2O)_x]^{++}$ ion (*7*). Also, poor deposits of zinc are obtained from anhydrous acetamide solutions, but bright plates result in the presence of traces of moisture (*8*). Perhaps water converts $[Y(en)_x]^{+3}$ species to more readily reducible $[Y(H_2O)_y]^{+3}$ species.

X-ray diffraction established the fact that the deposits were not rare-earth metal compounds. Very high "d" spacings in the lattices of the yttrium deposits were noted. The neodymium deposits appeared to be on the borderline between crystallinity and the amorphous state, and the lanthanum deposits were amorphous. Initial deposition in amphorous forms have been reported in other cases (*9*).

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Energy Transfer in Irradiated Solutions of Mixed Phosphors

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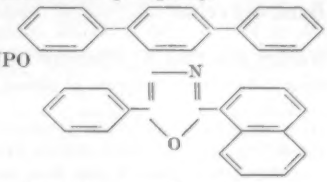
The field of scintillation counting for the determination of radioactivity is rapidly expanding. One of its most interesting developments has been the replacement of organic and inorganic crystals by solutions of selected organic phosphors. The high efficiencies obtained by Hayes (*1–3*) and his coworkers and by Arnold (*4*) in counting radiations from tritium and C^{14} with a toluene solution of diphenyloxazole (DPO) and alphanaphthylphenyloxazole (ANPO) indicate that mixtures of solutes are more efficient than single solutes. Mixtures of p-terphenyl



and ANPO

have been found to be highly efficient.

In the afore-mentioned studies, solutions containing 3 to 4 g of a primary phosphor such as p-terphenyl and about 0.05 g of a secondary phospher such as ANPO in 1 lit of toluene or xylene were used. The addition of this small amount of ANPO markedly increased the efficiency of the scintillation counter. This enhancement has been ascribed to an energy transfer from the primary to the secondary phosphor. Kallmann and Furst (*5*) proposed such a transfer. Bowen and his coworkers (*6*) have reported that at concentrations in the afore-stated range the shift of energy from the primary phosphor to the secondary phosphor is almost complete and the emission of light from the primary is almost completely eliminated.

Several explanations of the course of this energy transfer have been suggested (*5–8*). They generally are based on a process of successive energy transfer from a radioactive source to a solvent, to a primary phosphor, to a secondary phosphor, and to emitted light that is directed onto the photocathode of a photomultiplier tube. It seemed that an understanding of the energy-transfer process might be facilitated by a more straightforward experimental procedure.

Harrison (*9*) has reported that fluorescence under ultraviolet excitation and scintillation under radioactive excitation are identical phenomena. Therefore it was decided to investigate these energy transfers by normal ultraviolet spectrographic procedures (*10*). Accordingly a cylindrical quartz cell 2 cm in diameter was supported so that its flat end faced the slit of a Hilger quartz spectrograph. A UA-2 mercury lamp was placed so that light from it was incident at 45° on this flat surface. The rest of the cell was effectively shielded. By this means light that fails to penetrate the cell is reflected away from the slit of the spectrograph. Only a small amount of direct light from the mercury lamp enters the spectrograph. This light causes the narrow pips on the tracing in Fig. 1. These are characteristic lines of the mercury spectrum and serve as reference points.

The fluorescence spectra were recorded on Type 103-0 Kodak Spectrographic plates. Examination of the spectral images with a recording densitometer produced tracings, three of which are reproduced in Fig. 1. The number of grid spaces under a tracing appears to be an approximate factor for the comparison of the

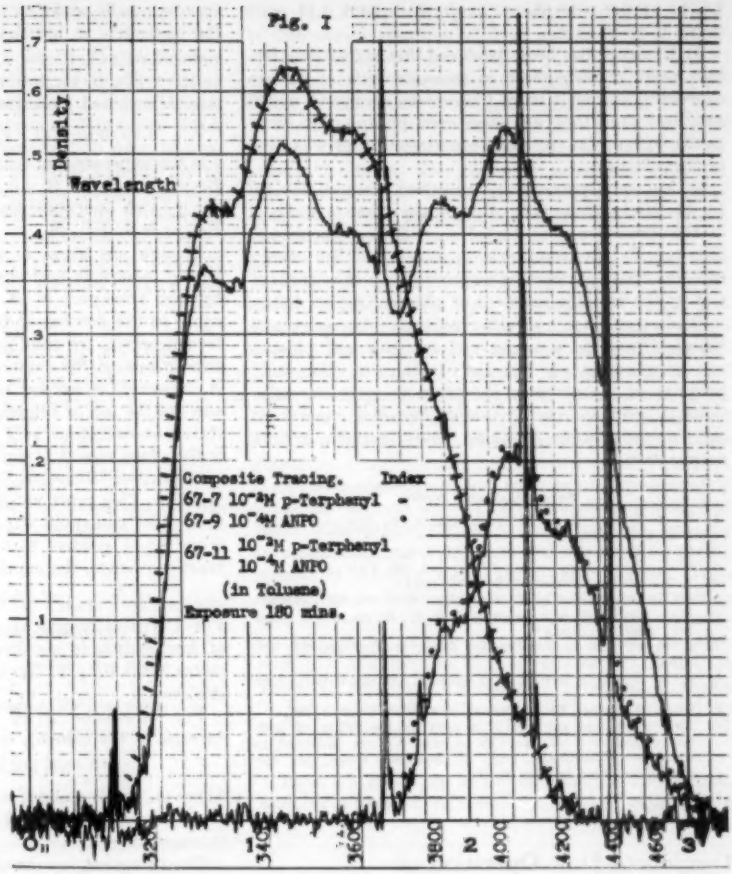


Fig. 1. Photoreproduction of a composite tracing of fluorescence spectra of (i) a solution of p-terphenyl in toluene, (ii) a solution of ANPO in toluene, and (iii) a solution of both p-terphenyl and ANPO in toluene. Indices have been added. The tracing representing the mixed phosphors shows the characteristic spectra of both phosphors. The height of the p-terphenyl peak is lowered a limited amount below that of the phosphor alone, whereas the ANPO peak is markedly enhanced.

energy of the fluorescence emitted by an irradiated solution. If one assumes that this relationship is quantitative, a count of the grid spaces under the tracings in Fig. 1 indicates that 8 percent more energy is emitted by a solution of mixed phosphors when the concentration of ANPO is $10^{-4}M$ and the p-terphenyl is $10^{-2}M$ than is emitted by these phosphors in separate solutions of these concentrations. When, however, the p-terphenyl concentration is $10^{-3}M$, the respective grid counts are about equal. When the p-terphenyl concentration is $5 \times 10^{-4}M$, the count for the combined phosphors is 22 percent less than it is when the phosphors are in separate solutions.

Although strictly quantitative comparisons have not yet been achieved, a few generalizations can be made.

1) The unique spectral characteristics of the separate phosphors persist when they are mixed.

2) The height of the ANPO peak is greatly enhanced in the mixed phosphor tracing. A slight amount of this enhancement, as well as the limiting broadening of the "shoulder" at about 3800 A, may have resulted from the overlap of the p-terphenyl and ANPO spectra. Study of many tracings reveals that the enhancement in the height of the ANPO peak is dependent on the concentrations of both phosphors.

3) The height of the p-terphenyl peak is reduced a slight amount by the addition of ANPO. This observation appears to differ radically from results reported in the literature (*5*, *6*) in which it is stated that the fluorescence of the primary phosphor is largely quenched. Those reports were based on results obtained when other phosphors were excited by radiation from a radioactive source. The emissions were generally designated as scintillations. If, as was noted earlier in this paper, scintillations and fluorescence are identical phenomena, it seems that a closer agreement should be expected.

4) The decrease of the height of the p-terphenyl tracing is a function of the ANPO concentration. In a number of toluene solutions whose ANPO concentration was $10^{-4}M$, and whose p-terphenyl concentration varied from $10^{-2}M$ to $10^{-5}M$, the decrease in the

height of the p-terphenyl peak was about 0.13 scale units in every case.

5) Evidence has been produced that shows that certain pairs of phosphors in some solvents at certain concentrations emit more fluorescent energy in mixed solutions than they do when each phosphor is alone in the solvent (at the same concentration as was used in the mixture).

6) A purely spectrographic method has been devised for the study of the fate of the energy that is involved in the fluorescence of liquid scintillation phosphors. This avoids the complexities introduced by energy transfer from radioactive emanations to the solutions and by the variable response of photomultiplier tubes.

Studies of these and other primary phosphors and "bandshifters" in a variety of solvents and plastics are being continued. The use of filters to allow only portions of the mercury spectrum to enter the solutions is proving most fruitful. Detailed reports of the various facets of these investigations are in preparation.

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Continuous-Flow Operation of Electroconvection Cells

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Electroconvective separation and purification of proteins is becoming more useful as a result of the availability of practical and efficient apparatus for this purpose. Recently two papers have appeared (*1, 2*) describing continuous-flow electroconvection for the fractionation of protein solutions. (Continuous-flow has also been practiced industrially for many years, chiefly for the purification of rubber latex (*3–6*).) Contrary to expectation, continuous-flow electroconvection is theoretically much slower and much less efficient than batch operation; this has also been confirmed experimentally.

Consider the batch operation in which V (ml) of solution containing C_A (g/ml) of A (at its isoelectric point) and C_B of B is placed in each reservoir. After a period t (hr) of electroconvection the upper reservoir still contains the original concentration of the isoelectric A, but the concentration of B has been reduced to yC_B in the upper reservoir and increased to $(2-y)C_B$ in the lower reservoir, where y is a fraction determined from the equation

$$t = 1.9\theta\int_{\epsilon}^{1-y}[(1-y)^{-5/3} - (1+y)^{-5/3}]^{5/4}dy; \qquad (1)$$

θ is the experimental transport constant as defined by Brown *et al.* (*7*). If y is taken as 0.001, $t = 5\theta$, so that for a batch operation the average rate of purification VC_A/t is $0.2VC_A/\theta$.

If now we begin to force buffer up through the bottom reservoir and channel at such a rate that the upward transport of B owing to buffer flow is just equal to the downward transport of B owing to electroconvection, the conditions at the top of the channel remain essentially constant insofar as B is concerned; A, however, is carried upward by the buffer stream and out the top of the cell, carrying with it the steady-state concentration yC_B of the B-component. For a buffer flow rate F (ml/hr), the upward buffer-flow transport of B is $2C_BF$ (g/hr) when y is a small fraction. This must not exceed the downward electroconvective transport VC_Bdy/dt. The derivative dy/dt from Eq. 1 reduces to $0.53y^{5/4}/\theta$ when y is a small fraction; hence, the maximum F is given by

$$F = 0.26Vy^{5/4}/\theta. \qquad (2)$$

For a typical system, if $y = 0.001$, the theoretical rate of purification in continuous-flow operation approximates $0.005\ VC_A/\theta$. The average batch rate is 40 times this rate.

For an amount A_t of the immobile component recovered in the effluent, conservation of mass gives

$$A_t = A_0(1 - e^{-Ft/V}), \qquad (3)$$

where A_0 is the amount initially present in the bottom reservoir and Ft is the total volume of buffer passed through the cell.

The foregoing equations are derived on the assumptions of the Kirkwood theory (*8*), specifically that of complete mixing in the reservoirs. In practice, mixing is far from complete; how far may be seen by comparing the foregoing equations with the experimental results obtained by Timasheff *et al.* (*2*). In their report 94 percent of BGG was recovered from a volume of 50 ml by passing 930 ml of buffer. The theoretical volume required is 140 ml; thus the mixing of the buffer stream with the reservoir contents was only 15 percent efficient.

The analysis of mixing in the bottom reservoir is further complicated by the fact that, although the concentration of the immobile component A remains uniform throughout the reservoir, the mobile component B concentrates as a result of electroconvection in a layer on the bottom of the reservoir, whereas the buffer stream, since it is less dense than the reservoir contents, tends to form a layer at the top of the reservoir. If the degree of mixing between these two layers is very small, as would be expected, the buffer entering the channel will not carry $2C_BF$ (g/hr) of the mobile component but a small fraction k of this amount. The

buffer flow rate F can then be increased without exceeding the desired carry-over of B.

Nonmixing greatly increases the over-all efficiency of the continuous-flow operation; it is nevertheless much slower and much less efficient than batch fractionation. For example, separating BGG and BSA by a three-stage batch electroconvection gave 94-percent recovery of 100-percent pure BGG in 200 ml of solution and 100-percent recovery of 97-percent pure BSA in 40 ml of solution; total time, 72 hr, as compared with 155 hr for the continuous-flow operation.

A more complete treatment, including an extension of the foregoing theory to other systems, is given elsewhere (*9*).

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10 May 1954.

Retention of Chromium by Glass Following Treatment with Cleaning Solution

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Over a period of years we have heard the remark that traces of chromium are retained on glassware that has been treated with the common laboratory cleaning solution of alkali metal dichromate and sulfuric acid. These comments frequently included cautions against possible interferences of the retained dichromate ions in precision measurements of surface tension and in some biological enzyme reactions. A literature search has revealed only one direct assay of the magnitude of this effect. In 1934, Laug made a colorimetric estimation of the retention of dichromate ion on several pieces of Pyrex brand and of lime glasses (*1*). He reported a few micrograms as $K_2Cr_2O_7$ in the wash waters following treatment by cleaning solution for 6 to 30 days. In view of the semiqualitative nature of this study and a limit of sensitivity of about 0.2 μg $K_2Cr_2O_7$, it was thought desirable to reconsider this problem in the light of modern radiochemical techniques.

Low-level counting techniques (*2*) and in particular the use of a stable sodium iodide scintillation spectrometer (*3*) for counting a gamma emitter such as Cr^{51} make possible detection sensitivities of orders of magnitude greater than the conventional colorimetric methods. High specific activity chromium chloride from the Atomic Energy Commission was converted to dichromate cleaning solution and used for a study of the retention of chromium on quartz, Pyrex brand, and commercial plate glass. The limit of detection by direct measurement of the glass surfaces was 10^{-4} μg as $K_2Cr_2O_7$, despite the fact that only 8 to 10 percent of the disintegrations of Cr^{51} are accompanied by gamma emission. The observed retentions exceeded this limit. It was, therefore, possible to count these samples by both the scintillation spectrometer and a Geiger counter.

The Cr^{51} was precipitated as the hydroxide and freed from chloride ion. The chromic ion was converted to dichromate by oxidation with excess hydrogen peroxide in basic solution. The excess peroxide was destroyed, and water and sulfuric acid were adjusted to approximate the usual cleaning solution (*4*). Some fused quartz disks 1½ in. in diameter and ¼ in. thick were on hand. Disks of the same dimensions were cut from Pyrex brand and commercial soda-lime plate glass.

In the initial experiment an area of approximately 7 cm^2 on one side only of these glasses was soaked at room temperature for 24 hr in radioactive cleaning solution. Each piece of glass was rinsed for about a minute in running water, dried, and counted. Tests showed that continued washing or strenuous rubbing with Kleenex did not alter the retention. The observed net counts per minute by the scintillation spectrometer (*5*) and by the Geiger counter are shown in Table 1. Although considerable variation is shown in Table 1, significantly more Cr^{51} was retained by the Pyrex brand glass.

Table 1. Retentions of Cr^{51} on glasses following treatment with cleaning solution (net counts per minute by Geiger counter and by scintillation spectrometer).

Instrument	Set	Fused quartz	Pyrex	Plate
Geiger counter	I	5	9	3
	II	5	12	7
Scint. spect. (Photo-peak)	I	60	229	33
	II	106	178	93

Two additional experiments were made to obtain information on the form of this retention. Samples of all three glasses were treated with a solution of chromium chloride in sulfuric acid containing the same specific activity of chromium as used in the dichromate cleaning solution. Also a sample of Pyrex brand glass was coated with grease from the face and hands and treated with the radioactive dichromate solution. These retentions are shown in Table 2.

The similarity between the data of Table 1 and set III of Table 2 suggests that the primary retention is adsorption of chromic ion rather than dichromate ion. Furthermore, the high retention on the greasy sample suggests either that adsorption of both ions occurs

Table 2. Retentions of Cr^{51} from Cr^{+6} solution and on greasy glass from cleaning solution.

Instrument	Set	Treatment	Fused quartz	Pyrex	Plate
Geiger counter	III	Cr^{+6}	4	5	5
	IV	Grease and $Cr_2O_7^{-2}$		124	
Scint. spect. (Photo-peak)	III	Cr^{+6}	84	85	126
	IV	Grease and $Cr_2O_7^{-2}$		3590	

much more readily on the surface of the grease or that partial oxidation of the grease film by $Cr_2O_7^{-2}$ causes the Cr^{+3} produced in the oxidation to be strongly affixed to the remaining film.

According to a standard assay that checked the specific activity reported by the Oak Ridge National Laboratory for the original solution, each count per minute by the scintillation spectrometer corresponds to 3×10^{-12} mole of chromium atoms. If a figure of 200 counts/min for Pyrex is taken from Table 1, the coverage is 2×10^{-10} mole/cm² of chromium atoms. If the area per atom is assumed to be 10^{-15} cm² and if the actual area is taken as twice the geometric area, the coverage is estimated as 3 percent of a monolayer.

It appears that a negligible amount, 0.03 monolayer or less, of chromium is retained by fused silica, Pyrex, or soda-lime glass following treatment by ordinary dichromate cleaning solution. On the other hand, if the cleaning is incomplete and the grease layer is not completely removed, as much as 0.5 monolayer of chromium may be retained.

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10 May 1954.

Action of Dimethylkynurenamine on Blood Pressure

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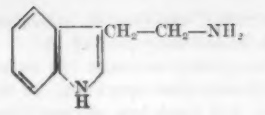
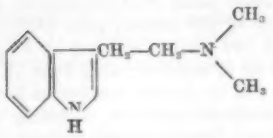
Bufotenine isolated from toad venom and serotonin from beef serum or mammalian gastrointestinal mucosa have remarkable pressor activity and vasoconstricting effect when injected intravenously into animals. Tryptamine (I) and dimethyltryptamine (II) also seem to have such an action, although it is weaker than serotonin itself.



(I)



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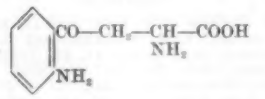

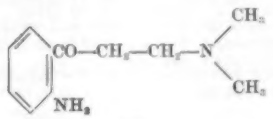

(III)

Recently dimethylkynurenamine (III) hydrochloride (mp 158° to 160°C, yellow needle or globe; monopicrate, mp 165°C, $C_{17}H_{19}O_8N_5$: calculated C, 48.45, H, 4.51, N, 16.63; found C, 48.65, H, 4.50, N, 16.05 percent), which was presumed to be derived from dimethyltryptamine or kynurenine, was prepared, in the presence of palladium charcoal, by hydrogenation of dimethyl o-nitrobenzoylethylamine hydrochloride (mp 165° to 167°C; picrate, mp 143°C, $C_{17}H_{17}O_{10}N_5$: calculated C, 45.23, H, 3.76, N, 15.52; found C, 45.62, H, 3.42, N, 15.32 percent).

This dimethylkynurenamine hydrochloride had a faint jasmin-like odor and was easily soluble in water, giving a yellow color. On paperchromatogram it was recognized as a blue fluorescent spot and showed with Dragendorff reagent a reddish-orange color, with p-dimethylaminobenzaldehyde in hydrochloric acid an orange color, and with diazotized sulfanilic acid a yellow color. Its Rf value developed with the supernatant of the mixture of acetic acid, butanol, and water in ratio 1 : 4 : 5 was 0.66, with 70 percent isopropanol 0.62. Its ultraviolet absorption spectrum has: λ_{max_1}, 256 mμ; λ_{max_2}, 358 mμ; λ_{min_1}, 246 mμ; λ_{min_2}, 280 mμ (*p*H 4.8).

Pharmacological study revealed that dimethylkynurenamine has a strong blood pressure-lowering activity in contrast to the pressor activity of tryptamine. When 100 μg of dimethylkynurenamine hydrochloride (per kilogram) was injected intravenously into the urethane-anesthetized rabbits (five rabbits, 1.5 to 2 kg), a lowering of blood pressure of 15 mm-Hg was recorded manometrically in the common carotid artery.

More remarkable action was seen with larger doses, as is indicated in Table 1.

Table 1. Blood pressure lowering activity of dimethylkynurenamine hydrochloride.

Doses (μg/kg)	Lowering of blood pressure (mm-Hg)	Approx. duration (min)
100	15	0.8 ~ 1
200	28	1 ~ 1.5
500	30	1.5 ~ 2
1000	35	3.5 ~ 4
3330	52	6 ~ 7

Although the action lowering the blood pressure of this amine hydrochloride was a little weaker than that of adenosine, a notable blood pressure-lowering substance, its inhibiting action on the epinephrine hypertension was somewhat stronger than that of the latter.

In our laboratory, we are now synthesizing 5-hydroxykynurenamine, which is supposed to have more remarkable action than kynurenamine and probably to be an antagonist of serotonin.

We wish to express our thanks to the Takeda Research Laboratory for making elementary analysis, and to A. Tashima, Pharmacological Department of Kumamoto University Medical School for helping with our pharmacological experiment. This work was aided by a grant from the Ministry of Education of Japan.

29 March 1954.

Natural Parthenogenesis in Turkey Eggs

M. W. Olsen and S. J. Marsden

Agricultural Research Service,
U.S. Department of Agriculture, Beltsville, Maryland

Research at the Agricultural Research Center, Beltsville, during the past three breeding seasons has shown that some eggs laid by nonmated and virgin Beltsville Small White turkeys may undergo a certain degree of embryonic development upon being incubated. This observation was first made during 1952 when it was found that 16.7 percent of 934 eggs laid by 29 nonmated turkeys showed some cellular proliferation when broken and examined after 7 days of incubation (*1*). These eggs were laid 42 to 224 days after the 29 females had been confined to a pen without males.

In 1953, 23 virgin Beltsville Small White turkey hens were placed on test. These hens had been segregated from their immature male pen mates at an early age, some at 4 wk and others at 12 wk. The first eggs were laid during January 1953, approximately 7 and 9 mo, respectively, after segregation from the immature males. During the ensuing 5-mo period, 1463 eggs were laid by the 23 virgin turkeys. In 14.1 percent of these, a delayed and abnormal type of development was found when the eggs were broken and examined after 7 to 9 days of incubation (*2*). In every instance development was not visible before a candling lamp until the fourth day of incubation. In contrast, normal embryos can be detected after 18 to 24 hr of incubation. A parthenogenetic embryo, when encountered at 9 days of incubation, had therefore attained the approximate size of a normal 5-day embryo.

During 1954, 79 virgin Beltsville White turkey hens were placed on test. These females were segregated from their immature male pen mates before 6 wk of age. At maturity they were placed in three pens, and all were given the same all-mash diet. Artificial lights were used in addition to normal daylight, the lights being turned on at 6 A.M. and off at 8 P.M. each day. The first eggs were laid during January 1954, approximately 8 mo after the birds had been isolated as young poults from their immature pen mates.

During the 8-wk period covered in this report (6 March–10 May 1954), the senior author was solely responsible for the care of the birds as well as for the gathering and incubation of the eggs. Since mated flocks of turkeys were being maintained in the same general area, special measures were taken to insure against mistaken identity of the eggs.

The procedure followed throughout the course of these studies was as follows. Each evening, all eggs laid by the virgin turkeys were placed in the incubator at a temperature of 99.5°F and at a relative humidity of 57 percent. Since earlier studies had shown that parthenogenetic development was not initiated before about 4 days of incubation (*1*), the eggs were candled for the first time on the ninth day. The eggs that on candling showed evidence of development were replaced in the incubator for an additional period of incubation. All eggs in which no development was visible on candling were removed and broken, and the disk of each was examined macroscopically for evidence of development. During the 8-wk period, 2537 eggs were laid by the 79 hens on test. Of this number, 568, or 22.4 percent, showed parthenogenetic development. In 492 of these 568 eggs, the development consisted solely of growth of the extraembryonic membranes. Even in the absence of an embryo, however, it was not unusual to find eggs in which a sheet of embryonic cells had covered almost the entire surface of the yolk. In 49 of the 568 eggs, differentiation proceeded to the extent that blood islands or blood vessels were clearly visible by candling or on macroscopic examination. In the remaining 27 of the 568 eggs, embryos as well as blood were identifiable on gross examination. These embryos attained various stages of development and are listed here in terms of equivalent development of the normal turkey embryo:

2 to 3 days:	8 embryos
4 to 6 days:	11 embryos
9 to 10 days:	3 embryos
14 to 18 days:	2 embryos
26 to 27 days:	3 embryos

With the exception of one individual, all embryos that had developed to or beyond the size of a normal

9-day embryo appeared to be perfectly formed. All except one of the advanced embryos were produced by daughters of a male with a history of retarded sexual maturity. Cytological studies indicate that the parthenogenetic embryos carried the diploid chromosome number (Yao, unpublished). In each of four instances where sex was determined, the embryo was found to be a male. These and other observations made during 1954 will be described in greater detail elsewhere.

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14 June 1954.

Communications

Education or Training?

The publication of William Seifriz's article on "A new university" [*Science* **120**, 87 (16 July 1954)] has impelled me to present this communication, the thesis of which is at least tangential to his. During the spring of 1954, I attempted in a very limited and informal manner to learn something of the cultural backgrounds of 15 candidates for the doctorate in botany, horticulture, agronomy, and zoology at the University of Illinois. My procedure was simple: at that point in the oral examination of each candidate at which the chairman of the examining committee places his palms on the table's edge, looks hopefully at the other members of the committee, and says, "Well, gentlemen, are there further questions?" I asked each candidate to identify as specifically as he could the following items; beneath each item are listed the numbers of satisfactory (S) and unsatisfactory (U) responses:

The Renaissance: S, 6; U, 9.
The Reformation: S, 5; U, 10.
The Monroe Doctrine: S, 2; U, 13.
Voltaire: S, 5; U, 10.
The Koran: S, 10; U, 5.
Plato: S, 7; U, 8.
The Medici Family: S, 1; U, 14.
Treaty of Versailles: S, 11; U, 4.
Bismarck: S, 4; U, 11.
Magna Carta: S, 2; U, 13.

I have made several interesting observations on these results: (i) Of the two students who gave an acceptably specific identification of the Monroe Doctrine, one was a Canadian. (ii) Only one student gave an acceptable identification of the Medici family; of the remaining 14 students, 10 had not even heard of the Medicis. (iii) Of the three students who turned in the best performances, one with 9 acceptable answers, two with 7 each, two were graduates of small liberal arts colleges. (iv) One student, a graduate of one of our largest state universities (*not* the University of Illinois!), failed to give a single acceptable answer. (v) The best score (9 acceptable answers) was that of a Canadian student (the other 14 were native-born citizens of the United States). (vi) Only two students were able to identify specifically Magna Carta, one of the great documents in the evolution of human political freedom; of the remaining 13, seven had not heard of Magna Carta. (vii) In a country in which the dominant religion is Christianity, twice as many students were able to identify the Koran as were able to give a satisfactory identification of the Reformation.

It is tempting to speculate upon the bases of these conclusions and upon their significance, but, since the sample was a small one (inevitably so, for the news of questions asked of doctoral candidates travels rapidly through the graduate student grapevine), one is justified in drawing but a single conclusion: perhaps we are overtraining both our graduate and undergraduate students to the detriment of their education.

One of my colleagues in history, interested in my little project, has, with my aid, drawn up a list of 10 notable theories, discoveries, and persons in the history of science and will try this list on doctoral candidates in the humanities to determine whether or not they are as poorly educated in science as my victims appear to have been in the humanities.

HARRY J. FULLER

Department of Botany,
University of Illinois, Urbana

5 August 1954.

Scarcity of Instrument Makers

During the past few years, much has been written about the scarcity of scientific personnel—a condition that may still exist but is becoming less acute.

Less attention, however, has been paid to another scarcity that is of longer duration and is, perhaps, more serious. I refer to the scarcity of instrument makers, the men who design and build the tools used by scientists, the "fine instruments" of long ago.

For years, we have been under the necessity of importing our scientific apparatus from Europe where the apprentice system has been—and still is—in general use. The apprentice served 4 years or more in the shop to learn the methods commonly used and then worked as journeyman or improver, learning more about his chosen profession in every shop.

In the United States the apprentice system is no more. Instrument manufacturers now employ toolmakers, lathe and milling machine hands, and so forth, who, while they may be excellent mechanics, know little about the simplest instrument, not even its use. The result is that no men are being trained in the

design, the skill to make, or the theory that is back of it all.

To become a competent instrument maker, a man must have a good education in mathematics, physics, chemistry, and mechanics, plus the ability to use this education, plus a high-grade manual skill. He should be able to use any instrument that comes to his attention for repair or rebuilding, or that he is called upon to manufacture. The instrument maker is a professional man in every sense of the word.

Since universities and colleges employ apparatus in vast numbers and the cost runs into large sums of money, it seems to me that these colleges ought to consider seriously the addition to their curriculums of a course in the theory and design of all kinds of apparatus, optical and otherwise. Included in this program should be a well-equipped instrument shop where theory would be translated into practice. By this means the college could supply its own requirements without purchasing outside. And the college should give thought to the additional prestige that would accrue, just as some colleges have a national reputation for their departments of law, medicine, engineering, chemistry, and the like.

Every college has among its students some who would like to take such a course if it were offered, Not every engineering graduate practices engineering, nor does every physicist follow physics as a means of earning a living.

During this time of economic unrest, and after world peace has been achieved, we shall want to do many things, accomplish many results more quickly and in a better manner than formerly. New and better methods of making high-precision measurements will be demanded, far beyond what we now have. Whereas physics and astronomy are now almost alone in making high-precision measurements, before long every branch of scientific endeavor will fall in line, and the demand for new instruments will be overwhelming. Our international relations and our involvement in global wars have made us acutely conscious of the need for better measuring instruments.

It is time that we Americans get busy and do some constructive thinking about this matter. And then, in the usual American fashion, do something about it.

FREDERICK A. BABCOCK

Precise Instrument Company,
Detroit 39, Michigan

10 September 1954.

"I Don't Want To Be Quoted"

The editorial "I don't want to be quoted by the press" [*Science*, 6 Aug.] is needed and should be helpful, especially to students and young scientists, whose habits are not yet fixed.

There are strong indications of a steadily lowering standard of ethics in the population of America, and world-history tells us what happens to nations when moral standards are persistently ignored. President Eisenhower once said: "America is great because she is good. When she ceases to be good she will cease to be great." Scientists, as teachers and in their messages to the public, can play a tremendous role here.

Statistics show that church membership is increasing in America much faster than population is increasing. Yet they also show that crime and wrongdoing of every kind are increasing faster than population is increasing. And, worst of all, the ages of delinquents and criminals are steadily lowering. Yet the teaching of every kind of crime and wrongdoing, in the most attractive forms, is steadily increasing. I refer especially to the so-called "comics" and "funnies" and to the flood of "murder mysteries" in Sunday supplements, magazines, and books. Sex crimes show an alarming increase, but some 100 pictures of nearly nude young women appear daily in newspapers, magazines, and books. America is ceasing to be good. History tells the results.

CARLETON R. BALL

Washington 15, D.C.

7 September 1954.

Role of the Salivary Glands in Extrathyroidal Iodine Metabolism

Recent evidence indicates that the parotid and submaxillary salivary glands function to control the level of thyroxin in the blood stream by deiodinating the hormone and recycling the iodide ion to the thyroid gland via the saliva and the gastrointestinal tract (*1–3*). This process of degradation appears to be essentially the reverse of the pathway of synthesis in the thyroid gland. The level of thyroxin in the blood is controlled by the rate of synthesis in the thyroid and the rate of degradation in the salivary glands.

If this concept is correct, it follows that the rate of degradation of thyroxin by the salivary glands will be under rigid control, since this is one of the methods for controlling the blood level of the hormone. However, if the process of degradation proceeds through the intermediates monoiodotyrosine and diiodotyrosine (DIT), then these substances must be very rapidly degraded to iodide, since they have not been shown to occur other than in thyroid tissue (*4*). This leads to the prediction that there should be a relatively slow, controlled deiodination of the thyroxin present in the blood stream and a very rapid deiodination of intravenously adminstered DIT.

Albert and his coworkers have studied the rate of deiodination of administered thyroxin and DIT in the human being and the rat (*5–7*). They found that thyroxin was deiodinated comparatively slowly in the body. DIT, on the other hand, was very rapidly degraded to the iodide ion. Tong, Taurog, and Chaikoff (*8*) studied the metabolism of labeled DIT in the rat and observed an extremely rapid deiodination of DIT, 90 percent being degraded to iodide in 20 min. The rate of deiodination was not influenced by previous thyroidectomy.

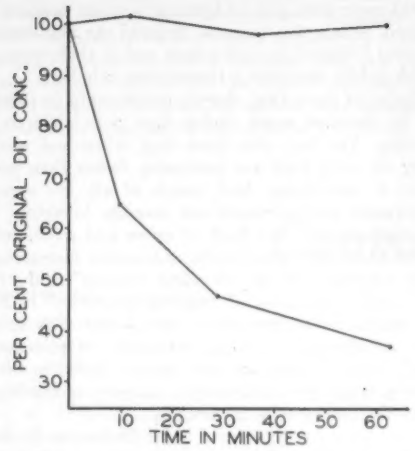

Fig. 1. Disappearance of intravenously injected DIT from the blood of salivariectomized (upper curve) and normal (lower curve) rats.

We have now been able to show that this rapid deiodination of intravenously administered DIT does not take place in rats from which the parotid, submaxillary, and sublingual salivary glands have been removed. Rats were placed under Nembutal anesthesia, and the submaxillary, parotid, and sublingual glands were removed. DIT (100 μg) labeled with I^{131} (100 μc) was then injected into the jugular vein, in physiological saline solution. A 0.5-ml aliquot of blood was collected from the tail-vein as soon as possible after the injection (usually about 2 min). This sample served to give zero-time values for iodide and DIT. Further 0.5-ml samples of blood were collected at convenient intervals. The heparinized blood samples were centrifuged, and aliquots of the resultant plasma were submitted directly to paper chromatographic analysis. The relative proportions of iodide ion and DIT on the chromatograms were determined by counting the appropriate spots with an end-window Geiger counter. The paper chromatographic and counting techniques have been described elsewhere (*1*).

Control rats were treated in exactly the same fashion, except that the salivary glands were left intact. The percentage activity as DIT in each plasma sample was compared with the percentage as DIT in the zero-time sample. In this way the results could be expressed as percentages of the original DIT concentration. The result of a typical experiment is shown in Fig. 1.

This evidence conclusively demonstrates that the salivary glands have a major role in the extrathyroidal metabolism of organic iodine in the body and that these organs function as "reverse thyroids."

D. M. Fawcett
S. Kirkwood

Department of Chemistry,
McMaster University, Hamilton, Ontario

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1 July 1954.

Rabies in Nonsanguivorous Bats of Texas

A survey conducted in nonsanguivorous bats of Texas has established two things: (i) the Mexican free-tailed bat (*Tadarida mexicana*) is a host for the virus of rabies in nature; and (ii) extensive inapparent infections occur in this species as evidenced by the development of neutralizing antibodies for the virus of rabies. Such immunity has been considered the result of naturally acquired inapparent infections. The opportunity to demonstrate such inapparent infection in bats in Texas was undertaken on an extensive scale on a military reservation because of a malady afflicting the bats.

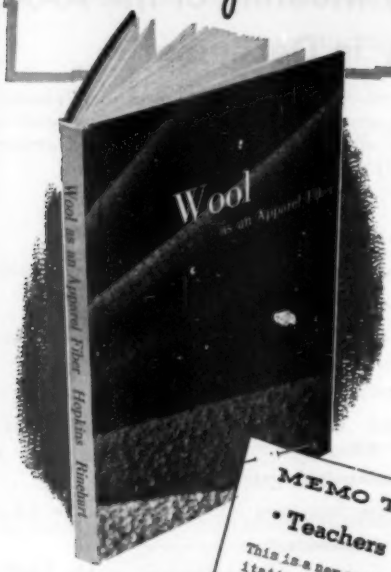
The bat isolates have been identified, and serologic studies have been accomplished by employing the standard neutralization technique and accepting as a minimum positive criterion the serums that will neutralize 100 LD_{50} of a fixed strain of rabies virus.

Kenneth F. Burns
Charles J. Farinacci

Lt. Col., VC, and Col., MC, Fourth Army Area Medical Laboratory, Brooke Army Medical Center, Fort Sam Houston, Texas

19 July 1954.

What do your students know about wool?



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1–3. Geological Soc. of America, annual, Los Angeles, Calif. (H. R. Aldrich, 419 W. 117 St., New York 27.)

1–3. Paleontological Soc., annual, Los Angeles, Calif. (K. E. Caster, Dept. of Geology, Univ. of Cincinnati, Cincinnati 21, O.)

1–5. National Metal Cong., Chicago, Ill. (C. L. Wells, 7301 Euclid Ave., Cleveland, O.)

3–4. Investment Casting Inst., Chicago, Ill. (V. S. Lazzara, 27 E. Monroe St., Chicago 3.)

3–5. American Crystallographic Assoc., and 12th annual Pittsburgh Diffraction Cong, Pittsburgh, Pa. (P. K. Koh, Allegheny Ludlum Research Laboratories, Alabama Ave., Brackenridge, Pa.)

3–5. Soc. of Rheology, Washington, D.C. (W. R. Willets, Titanium Pigment Corp., 99 Hudson St., New York 13.)

3–6. American Soc. of Parasitologists, Memphis, Tenn. (A. C. Walton, Dept. of Biology, Knox College, Galesburg, Ill.)

3–6. American Soc. of Tropical Medicine and Hygiene, Memphis, Tenn. (J. E. Larsh, Jr., School of Public Health, Univ. of North Carolina, Chapel Hill.)

4–5. East Coast Conf. on Airborne and Navigational Electronics, Baltimore, Md. (G. R. White, Bendix Aviation Corp., Towson 4, Md.)

4–5. Hawaiian Acad. of Science, Honolulu, Hawaii. (D. C. Cox, Experiment Station, HSPA, Honolulu 14.)

4–6. Air Pollution Control Assoc., Los Angeles, Calif. (E. S. Feldman, 2155 7 St., Los Angeles 23.)

4–6. American Acad. of Tropical Medicine, Memphis, Tenn. (E. H. Hinman, Univ. of Puerto Rico School of Medicine, San Juan 22.)

6. Committee for the Scientific Study of Religion, fall, Cambridge, Mass. (R. V. McCann, 48 Mt. Auburn St., Cambridge.)

7. American College of Dentists, Miami, Fla. (O. W. Brandhorst, 4221 Lindell Blvd., St. Louis, Mo.)

7–12. International Cong. of Military Medicine and Pharmacy, Luxemburg, Luxembourg. (A. R. Vernengo, Pozos 2045, Buenos Aires, Argentina.)

8–11. American Dental Assoc., annual, Miami, Fla. (H. Hillenbrand, 222 E. Superior St., Chicago 11, Ill.)

8–11. American Petroleum Inst., 34th annual, Chicago, Ill. (API, 50 W. 50 St., New York 20.)

8–11. Southern Soc. of Cancer Cytology, annual, St. Louis, Mo. (J. E. Ayre, 1155 N. W. 14 St., Miami, Fla.)

8–12. American Soc. of Agronomy, annual, St. Paul, Minn. (L. G. Monthey, 2702 Monroe St., Madison 5, Wis.)

8–12. Soil Conservation Soc. of America, St. Paul, Minn. (H. W. Pritchard, 1016 Paramount Bldg., Des Moines, Iowa.)

10–11. Conf. on Electrical Techniques in Medicine and Biology, Chicago, Ill. (E. D. Trout 4855 Electric Ave., Milwaukee, Wis.)

10–12. Field Emission Symposium, Pittsburgh, Pa. (R. Klein, Bureau of Mines, 4800 Forbes St., Pittsburgh 13.)

11–12. Corrosion Cong., Frankfurt, Germany. (DECHEMA, Frankfurt a.M., W. 13.)

(See issue of 17 September for more comprehensive listings.)

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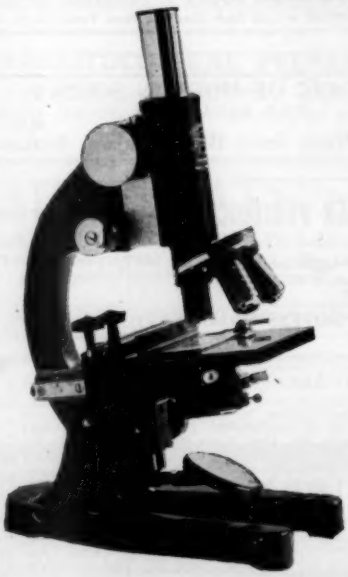

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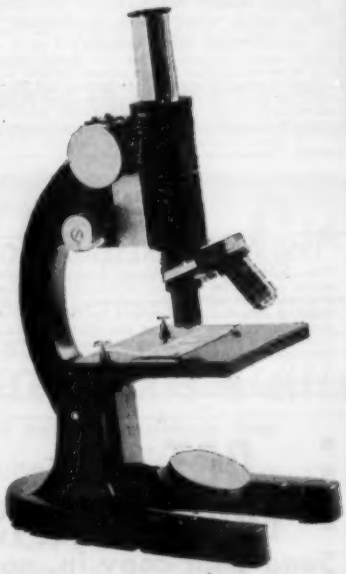

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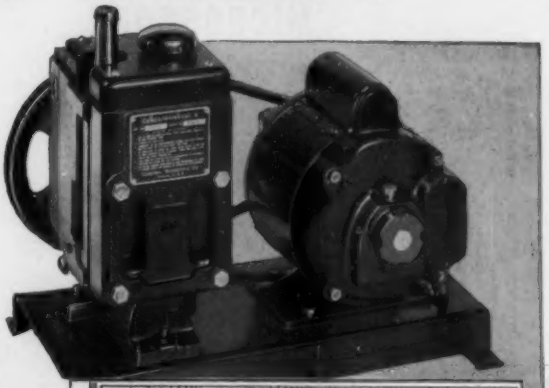

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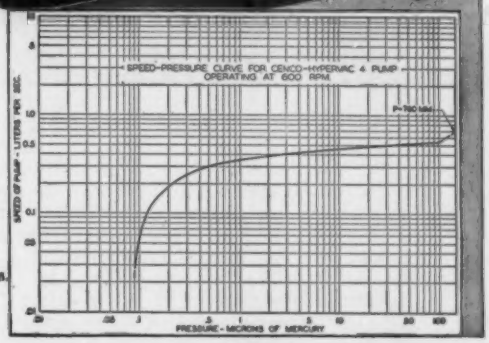
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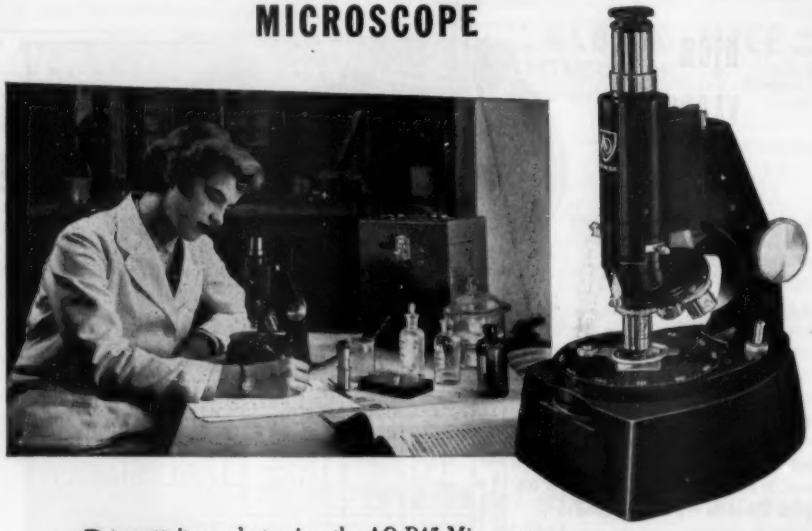

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